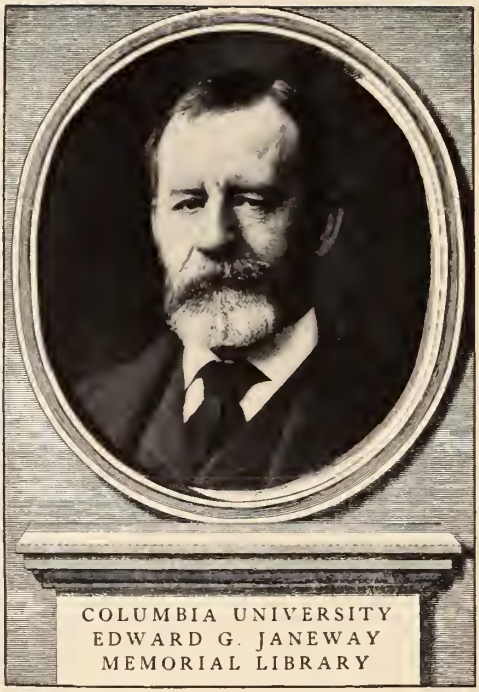
COLUMBIA LIBRARIES OFFSITE
HEALTH SCIENCES RESTRICTED
HR01306685

ERIAL
v. 15
A
1899



COLUMBIA UNIVERSITY
EDWARD G. JANEWAY
MEMORIAL LIBRARY

Digitized by the Internet Archive
in 2014

https://archive.org/details/transactionsofam1518amer

TRANSACTIONS

OF THE

AMERICAN CLIMATOLOGICAL ASSOCIATION.

FOR THE YEAR 1899.

VOLUME XV.

PHILADELPHIA:
PRINTED FOR THE ASSOCIATION.
1899.

Entered according to Act of Congress, in the year 1899, by
GUY HINSDALE, M.D., Secretary of the Association.

Copies may be had of the Secretary.

VOL. XI. contains a complete Index of 11 volumes.

DORNAN, PRINTER.

OFFICERS OF THE ASSOCIATION,

1899.

President.

BEVERLEY ROBINSON, M.D., New York.

Vice-Presidents.

JAMES A. HART, M.D., Colorado Springs.
RICHARD C. NEWTON, M.D., Montclair, N. J.

Secretary and Treasurer.

GUY HINSDALE, M.D., Philadelphia.

Council.

ISAAC HULL PLATT, M.D., New York.
S. E. SOLLY, M.D., Colorado Springs.
JAMES B. WALKER, M.D., Philadelphia.
E. FLETCHER INGALS, M.D., Chicago.
E. O. OTIS, M.D., Boston.

OFFICERS OF THE ASSOCIATION, 1900.

President.

ABRAHAM JACOBI, M.D., New York.

Vice-Presidents.

R. H. BABCOCK, M.D., Chicago.

J. W. BRANNAN, M.D., New York.

Secretary and Treasurer.

GUY HINSDALE, M.D., Philadelphia.

Council.

S. E. SOLLY, M.D., Colorado Springs.

JAMES B. WALKER, M.D., Philadelphia.

E. FLETCHER INGALS, M.D., Chicago.

E. O. OTIS, M.D., Boston.

BEVERLEY ROBINSON, M.D., New York.

Representative to the Executive Committee of the Congress of American Physicians and Surgeons for 1900.

FREDERICK I. KNIGHT, M.D., Boston.

ROLAND G. CURTIN, M.D., Philadelphia, *Alternate.*

CONTENTS.

	PAGE
List of Officers	vii
List of Members, 1899	ix
Minutes	xv
Constitution and By-Laws	xxix

President's Address. The High Aims of the Physician. By BEVERLEY ROBINSON, M.D., of New York 1
Light and Air in the Treatment of Consumption in Colorado. By CHARLES FOX GARDINER, M.D., of Colorado Springs, Colo. 19
Suggestions Concerning Early Diagnosis in Pulmonary Tuberculosis. By S. G. BONNEY, M.D., of Denver, Colo. . . 30
Some Notes Upon the Tuberculin Test. By EDWARD O. OTIS, M.D., of Boston 42
Subsequent Histories of Arrested Cases of Phthisis Treated at the Sharon Sanitarium. By VINCENT Y. BOWDITCH, M.D., of Boston 53
Röntgen Ray Examinations in Incipient Pulmonary Tuberculosis. By FRANCIS H. WILLIAMS, M.D., of Boston . . 68
Intermediate Altitude for the Consumptive Invalid. By B. P. ANDERSON, M.D., of Colorado Springs, Colo. . . . 87
Why Fumigation of Apartments Occupied by Tuberculous Patients at Health Resorts Should be Under Municipal Control. By CHARLES F. MCGAHAN, M.D., of Aiken, S. C. . 94
Some Remarks on Climate and Renal Diseases. By JAMES B. WALKER, M.D., of Philadelphia 99
Climate as it Affects the Skin and its Diseases. By L. DUNCAN BULKLEY, A.M., M.D., of New York 108
Hygienics of the Skin. By L. D. JUDD, M.D., of Philadelphia 118
The Climatology of Nudity. (Light, Heat, Atmosphere.) By WILLIAM DUFFIELD ROBINSON, M.D., of Philadelphia . 128
Hydrotherapy in the Treatment of Insomnia. By IRWIN H. HANCE, M.D., of Lakewood, N. J. 137

PAGE

Recent Inquiries Concerning the Blood Changes Induced by Altitude. By S. E. Solly, M.D., of Colorado Springs, Colo. 144

High Altitude and Heart Disease, with Report of Cases. By Robert H. Babcock, M.D., of Chicago 159

Prognosis in Chronic Valvular Affections of the Heart. By N. S. Davis, Jr., A.M., M.D., of Chicago 174

Treatment of Cardiac Asthenia of Pneumonia. By Henry L. Elsner, M.D., of Syracuse, N. Y. 187

Remarks Based Upon a Further Experience with Calomel in Diphtheria. By L. D. Judd, M.D., of Philadelphia . . 197

Cardio-œsophageal Gush and Click. By Roland G. Curtin, M.D., of Philadelphia 203

The Bearing of Embolism on the Course of Valvular Heart Disease. By William M. Gibson, M.D., of Utica, N. Y. . 209

A Case of Aneurism of the Aorta. By Thomas Darlington, M.D., of New York 218

Empyema from a Surgical Stand-point. By John C. Munro, M.D., of Boston 220

Traumatic Ruptures of the Heart, with a Case. By Richard Cole Newton, M.D., of Montclair, N. J. 226

Functional Cardiac Murmurs. By A. Jacobi, M.D., LL.D., of New York 240

Bicycling in its Relation to Heart Disease. By A. C. Getchell, M.D., of Worcester, Mass. 256

The Effects of Violent and Prolonged Muscular Exercise Upon the Heart. By Harold Williams, M.D., of Boston, Mass., and Horace D. Arnold, M.D., of Boston, Mass. 267

The Relation of Local Meteorological Conditions to the Influenza Epidemic in Philadelphia, Winter of 1898–99. By Howard S. Anders, A.M., M.D., of Philadelphia . . 286

The Cold Wave of February, 1899. By Guy Hinsdale, M.D., of Philadelphia 304

William Pepper.

LIST OF OFFICERS.

Presidents.

Name.	Year.
A. L. Loomis	1884–5.
William Pepper	1886.
Frank Donaldson	1887.
A. L. Loomis	1888.
V. Y. Bowditch	1889.
Charles Denison	1890.
F. I. Knight	1891.
W. E. Ford	1892.
R. G. Curtin	1893.
A. H. Smith	1894.
S. E. Solly	1895.
J. B. Walker	1896.
E. Fletcher Ingals	1897.
E. O. Otis	1898.
Beverley Robinson	1899.
Abraham Jacobi	1900.

Vice-Presidents.

F. I. Knight, W. H. Geddings	1884–5.
Frank Donaldson, Beverley Robinson	1886.
V. Y. Bowditch, R. G. Curtin	1887.
A. Y. P. Garnett, J. T. Whittaker	1888.
J. R. Leaming, E. T. Bruen	1889.
A. L. Gihon, H. B. Baker	1890.
E. L. Trudeau, T. S. Hopkins	1891.
E. Fletcher Ingals, Beverley Robinson	1892.
A. H. Smith, E. O. Otis	1893.
I. Hull Platt, E. L. Trudeau	1894.
John H. Musser, G. R. Butler	1895.
Charles E. Quimby, James A. Hart	1896.
S. A. Fisk, John C. Munro	1897.
Beverley Robinson, C. F. McGahan	1898.
James A. Hart, R. C. Newton	1899.
R. H. Babcock, John Winters Brannan	1900.

Secretaries and Treasurers.

James B. Walker	1884–95.
Guy Hinsdale	1895–1900.

LIST OF MEMBERS.

Honorary Members.

ELECTED

1890. Stillé, Alfred, 3900 Spruce Street, Philadelphia.
1897. Weber, Hermann, 10 Grosvenor Street, W., London, England.
1897. Williams, Charles Theodore, 2 Upper Brook Street, W., London.

Corresponding Members.

1898. Eyre, G. G., Claremont, Cape Town, South Africa.
1898. Gache, Samuel, 729 Corrientes Street, Buenos Ayres, South America.
1898. Licéaga, Eduardo, 4 San Andres Street, Mexico.
1898. Orvañanos, Domingo, 25 Chavarria Street, Mexico.
1898. Ruedi, Carl, Arosa, Switzerland.
1898. Sunderland, Septimus, 11 Cavendish Place, W., London.
1898. Wragge, Clement L., Brisbane, Queensland, Australia.

Active Members.

1888. Abbot, Griffith E., 13½ Street and Pennsylvania Avenue, Washington, D. C.
1897. Alden, C. H., Assistant Surgeon-General, U. S. A., Washington, D. C.
1897. Alton, Charles D., 86 Farmington Avenue, Hartford, Conn.
1898. Anders, Howard S., 1836 Wallace Street, Philadelphia.
1889. Anders, J. M., 1605 Walnut Street, Philadelphia.
1890. Anderson, B. P., Colorado Springs, Col.
1890. Atkins, Francis H., East Las Vegas, N. M.

ELECTED

1893. Babcock, R. H., 103 State Street, Chicago.
1885. Baker, Henry B., 726 Ottawa Street, Lansing, Mich.
1898. Baldwin, Edward R., Saranac Lake, N. Y.
1898. Battle, S. Westray, Asheville, N. C.
1885. Bell, A. N., 337 Clinton Street, Brooklyn.
1896. Bergey, David H., Laboratory of Hygiene, University of Pennsylvania, Philadelphia.
1896. Bernardy, E. P., 221 South 17th Street, Philadelphia.
1897. Billings, Frank, 100 State Street, Chicago.
1897. Blackader, Alexander D., 236 Mountain Street, Montreal, Canada.
1895. Boardman, W. S., 57 Hancock Street, Boston.
1897. Bonney, S. G., 726 14th Street, Denver.
1884. Bosworth, F. H., 41 Park Avenue, New York.
1885. Bowditch, V. Y., 506 Beacon Street, Boston.
1895. Brandt, C. N., Hot Springs, Va.
1891. Brannan, John W., 11 West 12th Street, New York.
1894. Bridge, Norman, 217 South Broadway, Los Angeles, Cal.
1898. Brower, D. R., 597 Jackson Boulevard, Chicago.
1897. Brown, Sanger, Reliance Building, Chicago.
1890. Buckley, J. J., Missoula, Mont.
1896. Bulette, W. W., Central Block, Pueblo, Col.
1898. Bulkley, L. D., 4 East 37th Street, New York.
1886. Butler, G. R., 229 Gates Avenue, Brooklyn.

1896. Campbell, W. A., 38 Bank Building, Colorado Springs.
1898. Casselberry, W. E., 103 State Street, Chicago.
1894. Chapin, Frederick W., Hot Springs, Va.
1887. Chapman, S. H., New Haven, Conn.
1898. Chappell, Walter F., 7 East 55th Street, New York.
1898. Cleemann, Richard A., 2135 Spruce Street, Philadelphia.
1894. Coleman, Thomas D., 563 Green Street, Augusta, Ga.
1889. Coolidge, A., Jr., 613 Beacon Street, Boston.
1885. Curtin, R. G., 22 South 18th Street, Philadelphia.

1892. Daland, Judson, 317 South 18th Street, Philadelphia.
1885. Daly, W. H., 516 Market Street, Pittsburg, Pa.

ELECTED

1890. Darlington, Thomas, Jr., King's Bridge, New York City.
1897. Davis, N. S., Jr., 65 Randolph Street, Chicago.
1884. Denison, Charles, 823 14th Street, Denver.
1897. De Witt, Calvin, Surgeon U. S. A., Fortress Monroe.
1884. Didama, H. D., 424 South Salina Street, Syracuse, N. Y.
1890. Dodge, H. O., Boulder, Colorado.
1898. Dudley, E. C., 1619 Indiana Avenue, Chicago.
1896. Dudley, Wm. F., 147 Clinton Street, Brooklyn.

1897. Edson, Carroll E., McPhee Building, Denver, Col.
1892. Elsner, H. L., Fayette Park, Syracuse, N. Y.
1885. Eskridge, J. T., 204 Equitable Building, Denver, Col.

1887. Fisk, Samuel A., 37 18th Street, Denver, Col.
1885. Ford, Willis E., 266 Genesee Street, Utica, N. Y.
1885. French, Thomas R., 469 Clinton Avenue, Brooklyn.
1897. Fütterer, Gustav, 34 Washington Street, Chicago.

1896. Gardiner, C. F., 224 Pike's Peak Avenue, Colorado Springs, Col.
1886. Garnett, A. S., Hot Springs, Ark.
1898. Getchell, Albert C., 6 Linden Street, Worcester, Mass.
1892. Gibson, William M., 260 Genesee Street, Utica, N. Y.
1884. Glasgow, W. C., 2847 Washington Avenue, St. Louis.
1893. Gray, Landon Carter, 6 East 49th Street, New York.

1893. Hance, I. H., Lakewood, N. J.
1896. Hare, Hobart A., 222 South 15th Street, Philadelphia.
1891. Hart, James A., Colorado Springs, Col.
1896. Heffron, John L., 528 South Salina Street, Syracuse, N. Y.
1893. Hinsdale, Guy, 3943 Chestnut Street, Philadelphia.
1885. Hopkins, Thomas S., Thomasville, Ga.

1884. Ingals, E. Fletcher, 34 Washington Street, Chicago.

1889. Jacobi, A., 110 West 34th Street, New York.
1888. Jayne, W. A., 217 McPhee Building, Denver, Col.

ELECTED

1897. Johnson, Frank S., 2521 Prairie Avenue, Chicago.
1886. Johnston, W. W., 1603 K Street, N. W., Washington, D. C.
1893. Judd, L. D., 3603 Powelton Avenue, Philadelphia.

1890. Kellogg, J. H., Battle Creek, Mich.
1899. Klebs, Arnold C., 600 State Street, Chicago.
1884. Knight, Frederick I., 195 Beacon Street, Boston.

1887. Langmaid, S. W., 373 Boylston Street, Boston.
1899. Le Fevre, Egbert, 52 West 56th Street, New York.
1890. Lincoln, R. P., 32 West 31st Street, New York.
1896. Loomis, Henry P., 58 East 34th Street, New York.

1894. McGahan, C. F., Aiken, S C., and Bethlehem, N. H.
1887. Mays, Thomas J., 1829 Spruce Street, Philadelphia.
1898. Merrick, Samuel K., 843 N. Eutaw Street, Baltimore.
1899. Minor, Charles L., Asheville, N. C.
1891. Moore, H. B., Colorado Springs, Col.
1889. Munro, John C., 173 Beacon Street, Boston.
1886. Musser, John H., 1927 Chestnut Street, Philadelphia.
1890. Mulhall, J. C., 3561 Olive Street, St. Louis.

1895. Newton, R. C , 42 Church Street, Montclair, N. J.
1899. Norrie, V. H., 21 West 37th Street, New York.
1888. Nunn, Richard J., 119 York Street, Savannah.

1884. Orme, H. S , 175 North Spring Street, Los Angeles, Cal.
1888. Otis, E. O., 308 Commonwealth Avenue, Boston.

1887. Peale, A. C , 605 12th Street, N. W., Washington, D. C.
1893. Peterson, Frederick, 60 West 50th Street, New York.
1895. Phillips, W. F. R., Weather Bureau, Washington, D. C.
1885. Platt, Isaac Hull, 30 West 71st St., New York.
1887. Platt, Walter B., 802 Cathedral Street, Baltimore.

1891. Quimby, Charles E., 44 West 36th Street, New York.

1891. Ransom, C. C., 152 West 48th Street, New York (Richfield Springs).

ELECTED

1884. Reed, Boardman, 1928 Chestnut Street, Philadelphia.
1885. Rice, C. C., 123 East 19th Street, New York.
1893. Risley, S. D., 1722 Walnut Street, Philadelphia.
1899. Rives, William C., 289 Madison Avenue, New York.
1884. Robinson, Beverley, 42 West 37th Street, New York.
1890. Robinson, W. D., 2012 Mt. Vernon Street, Philadelphia.
1892. Roe, John O., 28 North Clinton Street, Rochester, N. Y.
1890. Rogers, E. J. A., 222 Colfax Avenue, Denver, Col.
1889. Ruck, Carl von, Asheville, N. C.

1884. Schauffler, E. W., 1221 Washington Street, Kansas City, Mo.
1884. Shurly, E. L., 32 Adams Avenue, West Detroit, Mich.
1890. Smith, A. Alexander, 8 West 47th Street, New York.
1885. Smith, Andrew H., 18 East 47th Street, New York.
1887. Smith, Frank Fremont, Palm Beach, Florida, and Bar Harbor, Maine.
1887. Solly, S. E., 2 North Cascade Avenue, Colorado Springs, Colorado.
1898 Stubbert, J. Edward, Liberty, New York.

1892. Taylor, H. Longstreet, 75 Lowry Arcade, St. Paul, Minn.
1896. Taylor, J. Madison, 1504 Pine Street, Philadelphia.
1885. Trudeau, E. L., Saranac Lake, N. Y.
1884. Tyndale, J. Hilgard, 13th and P Streets, Lincoln, Neb.
1898. Tyson, James, 1506 Spruce Street, Philadelphia, Pa.

1884. Walker, James B., 1617 Green Street, Philadelphia.
1891. Watson, E. W., 131 North 20th Street, Philadelphia.
1895. Weber, Leonard, 25 West 46th Street, New York.
1897. Whitcomb, H. H., Norristown, Pa.
1898. Whitney, Herbert B., 726 14th Street, Denver, Col.
1898. Williams, Francis H., 505 Beacon Street, Boston.
1898. Williams, Harold, 528 Beacon Street, Boston.
1885. Williams, H. F., 197 Gates Avenue, Brooklyn.
1884. Wilson, James C , 1437 Walnut Street, Philadelphia.

Total, 130 active members.

MINUTES.

The Sixteenth Annual Meeting of the Association was convened in the Hall of the New York Academy of Medicine, on May 9, 1899, at 10.30 A.M., the President, Dr. Beverley Robinson, of New York, in the chair.

The following members were present and took part in the various proceedings:

Col. C. H. Alden, U. S. A., Washington, D. C.
Dr. Charles D. Alton, Hartford.
Dr. Howard S. Anders, Philadelphia.
Dr. B. P. Anderson, Colorado Springs.
Dr. R. H. Babcock, Chicago.
Dr. Edward R. Baldwin, Saranac Lake.
Dr. A. N. Bell, Brooklyn.
Dr. E. P. Bernardy, Philadelphia.
Dr. Sherman G. Bonney, Denver.
Dr. F. H. Bosworth, New York.
Dr. V. Y. Bowditch, Boston.
Dr. John W. Brannan, New York.
Dr. L. D. Bulkley, New York.
Dr. Glentworth R. Butler, Brooklyn.
Dr. Walter F. Chappell, New York.
Dr. Richard A. Cleemann, Philadelphia.
Dr. Thomas D. Coleman, Augusta.
Dr. Roland G. Curtin, Philadelphia.
Dr. Judson Daland, Philadelphia.
Dr. Thomas Darlington, New York.
Dr. N. S. Davis, Jr., Chicago.
Dr. H. L. Elsner, Syracuse.
Dr. Samuel A. Fisk, Denver.
Dr. Thomas R. French, Brooklyn.
Dr. Charles Fox Gardiner, Colorado Springs.

Dr. Albert C. Getchell, Worcester.
Dr. I. H. Hance, Lakewood.
Dr. Hobart A. Hare, Philadelphia.
Dr. John L. Heffron, Syracuse.
Dr. Guy Hinsdale, Philadelphia.
Dr. E. Fletcher Ingals, Chicago.
Dr. A. Jacobi, New York.
Dr. L. D. Judd, Philadelphia.
Dr. F. I. Knight, Boston.
Dr. S. W. Langmaid, Boston.
Dr. R. P. Lincoln, New York.
Dr. H. P. Loomis, New York.
Dr. Egbert Le Fevre, New York.
Dr. C. F. McGahan, Aiken.
Dr. John C. Munro, Boston.
Dr. R. C. Newton, Montclair, N. J.
Dr. E. O. Otis, Boston.
Dr. A. C. Peale, Washington, D. C.
Dr. W. F. R. Phillips, Washington, D. C.
Dr. Isaac Hull Platt, New York.
Dr. Charles E. Quimby, New York.
Dr. C. C. Ransom, New York.
Dr. Boardman Reed, Philadelphia.
Dr. C. C. Rice, New York.
Dr. S. D. Risley, Philadelphia.
Dr. Beverley Robinson, New York.
Dr. W. D. Robinson, Philadelphia.
Dr. John O. Roe, Rochester, N. Y.
Dr. A. Alexander Smith, N. Y.
Dr. S. E. Solly, Colorado Springs.
Dr. James B. Walker, Philadelphia.
Dr. Leonard Weber, New York.
Dr. H. H. Whitcomb, Norristown, Pa.
Dr. Francis H. Williams, Boston.
Dr. Harold Williams, Boston.
Dr. H. F. Williams, Brooklyn.

The President: Before I read my opening address, I wish to say that the Association must feel especially happy that we have not lost any of our members this past year by death. At our last meeting we were obliged to listen to the notice of the loss

of some of our most faithful and worthy members, as well as one who was most distinguished in our profession.

Opening address by the President, Dr. Beverley Robinson, of New York. Subject: "The High Aims of the Physician."

The President: I would like to call attention to the recommendation of Council, that the papers which are presented should not exceed fifteen minutes, and that the members who participate in the discussion should be limited to five minutes, unless special permission be granted. I would like to ask what the pleasure of the Association is in regard to this matter?

Dr. Brannan: We have so many papers on our programme that I move that members who take part in the discussion be limited to five minutes.

Seconded by Dr. Otis.

Dr. Brannan: It has been suggested that I amend my motion so as to include in it that the length of papers be limited to fifteen minutes.

Amendment seconded by Dr. Otis. Carried.

Dr. Curtin: I move that visitors be allowed to take part in the discussions.

Seconded. Carried.

Dr. Charles F. Gardiner read a paper on the "Treatment of Consumption by Air and Light in Colorado."

Discussed by Drs. Phillips, Otis, W. D. Robinson, N. S. Davis, Jr., S. A. Fisk, and S. E. Solly.

Dr. S. G. Bonney read a paper entitled "Suggestions Concerning Early Diagnosis in Pulmonary Tuberculosis."

Discussed by Drs. Knight, Roe, Babcock, Curtin, Newton, Ingals, McGahan, Otis, Phillips, Langmaid, and Hinsdale.

Dr. E. O. Otis read "Notes on the Tuberculin Test."

Discussed by Drs. Langmaid, Otis, and F. H. Williams.

Dr. V. Y. Bowditch read "Subsequent Histories of Arrested Cases of Phthisis Treated at the Sharon Sanitarium."

Discussed by Drs. Stubbert, Newton, F. H. Williams, V. Y. Bowditch and E. R. Baldwin.

Dr. F. H. Williams read a paper entitled "Röntgen Ray Examinations in Incipient Pulmonary Tuberculosis."

Business Meeting at 1 p.m.

Dr. Brannan, Chairman of Committee of Arrangements, made announcements regarding the luncheon served in the Academy between sessions, and the smoker at Delmonico's, both tendered by the New York members; the dinner at the Hotel Manhattan, and the special train to Liberty and return, on May 11th.

The Secretary and Treasurer then read the following report: Since the last report the Secretary has edited and had printed the Fourteenth Volume of the Transactions of the Association. It has been distributed to the members and to seventy-five libraries and medical journals in this country and abroad. As usual, we make acknowledgment to the International Bureau of Exchanges of the Smithsonian Institution for the free distribution of our copies to foreign lands. The volume cost the Association $453.27. We are under obligations to our printer, Mr. W. J. Dornan, for the prompt and efficient way in which he conducts his portion of the work.

The Treasurer's books are herewith presented, with accompanying vouchers, and show a balance, with all bills paid to date, of $176.92.

Of the 130 active members, we have lost none by death since we met last. The following resignations have been presented: Drs. A. C. Abbott, I. N. Danforth, J. N. Hyde, H. H. Schroeder. Three members are delinquent more than two years. Seven names are presented this year for election to membership on the active list. Full information has been forwarded to each member regarding the candidates.

At the meeting of the Council in New York City, December 28, 1898, it was recommended that the Association place a limit to the number of active members at 150.

The changes in the Constitution proposed by resolution at the last meeting were approved, and may be acted upon at the present meeting, all members having been duly notified of the same. They are as follows:

Resolved, That the Constitution be changed so that Article IV., Section 1, shall read: The officers shall consist of a President, two Vice-Presidents, a Secretary and Treasurer, who, with five other members and the delegate and alternate to the Executive

Committee of the Congress of American Physicians and Surgeons, shall constitute the Council of the Association.

Resolved, That Article III., Section 2, be amended by the insertion of the words "whose applications have been" before the words "indorsed by two active members," etc.

We have been requested by the Executive Committee of the Congress of American Physicians and Surgeons to appoint a member of the Committee of Arrangements, to represent this Association in the Committee of Arrangements for the Congress of 1900.

It was moved by Dr. Newton, and seconded by Dr. Walker, that the report of the Secretary and Treasurer be accepted and approved. Carried.

The President then appointed Drs. Otis and Coleman to audit the accounts.

The President appointed Drs. Knight, Solly, Curtin, McGahan, and Quimby as the Nominating Committee.

The President appointed Dr. W. W. Johnston a member of the Committee of Arrangements for the Congress of American Physicians and Surgeons, at Washington in 1900.

It was then moved and seconded that Article IV., Section 1, and Article III., Section 2, of the Constitution be amended as stated in the Secretary's report. Carried.

Dr. Knight inquired as to the recommendation of the Council as to the limit to be placed on the number of active members.

Dr. Newton spoke in favor of the same. Drs. Knight and Walker spoke against it.

A motion was then made and seconded that 150 be fixed as the limit of the number of active members. Motion lost. The list of propositions for active membership was then read, and the business meeting was adjourned. After luncheon the Council met at 2.30 P.M.

The Association was called to order at 3 P.M.

Dr. B. P. Anderson read a paper on "Intermediate Altitude for the Consumptive Invalid."

Discussed by Drs. Leonard Weber, Solly, and Newton.

Dr. C. F. McGahan read a paper entitled "Why Fumigation of Apartments Occupied by Tuberculous Patients at Health Resorts Should be Under Municipal Control."

Discussed by Drs. Otis, Gardiner, Hinsdale, and McGahan.

Dr. Walker read a paper on "Climate in Relation to Renal Diseases."

Discussed by Drs. Baldwin, Daland, Newton, Phillips, Fisk, Henry Sewall (by invitation), Bulkley, Jacobi, N. S. Davis, Jr., and Walker.

Dr. L. D. Bulkley read a paper on "Climate as it Affects the Skin and its Diseases."

Discussed by Drs. Solly, Jacobi, Ransom, and Daland.

Dr. L. D. Judd read a paper on "Hygienics of the Skin."

Discussed by Drs. Bulkley, Babcock, Ransom, Walker, Solly, Daland, Langmaid, and Judd.

Dr. I. H. Hance read a paper on "Hydrotherapy in the Treatment of Insomnia."

Discussed by Drs. Ransom and Hance.

SECOND DAY.—SESSION AT 10 A.M.

Dr. W. D. Robinson read a paper on the "Climatology of Nudity."

Discussed by Drs. W. F. R. Phillips, Solly, and Newton.

Dr. Solly read a paper on "Recent Inquiries Concerning the Blood Changes Induced by Altitude."

Discussed by Drs. Otis, Jacobi, Daland, Phillips, and Solly.

Dr. Babcock read a paper on "High Altitude and Heart Disease."

Discussed by Drs. Sewall, Quimby, and Babcock.

Dr. A. Jacobi read a paper on "Functional Cardiac Murmurs."

Discussed by Drs. H. S. Anders, E. F. Ingals, J. C. Munro, Harold Williams, and Jacobi.

Dr. N. S. Davis, Jr., read a paper on "Prognosis in Chronic Valvular Affections of the Heart."

Discussed by Drs. Curtin, Walker, Babcock, Jacobi, and Knight.

Dr. Elsner read a paper on the "Cardiac Asthenia of Pneumonia."

Discussed by Drs. Walker, Curtin, Daland, Babcock, Jacobi, and Elsner.

Dr. Curtin read a paper on "Œsophageal Gush and Click as a Cause of a Simulated Heart Murmur."

Discussed by Dr. F. I. Knight.

Dr. J. C. Munro read a paper on "Empyema from a Surgical Stand-point."

Discussed by Drs. Harold Williams, V. Y. Bowditch, R. G. Curtin, and Elsner.

The session was adjourned at 12.30 for the business meeting.

The Nominating Committee made its report, with the following recommendations:

For President, Dr. A. Jacobi, of New York.

For Vice-Presidents, Dr. R. H. Babcock, of Chicago, and Dr. John Winters Brannan, of New York.

For Secretary and Treasurer, Dr. Guy Hinsdale, of Philadelphia.

For Member of Council, to serve five years, Dr. Beverley Robinson, of New York.

There being no other nominees, it was then moved and seconded that the Secretary cast a ballot for the list as read.

This was done, and the foregoing officers were declared elected for the ensuing year.

The President-elect was then escorted to the President's right and presented to the Association.

The Association then proceeded to the election of new members. The following were elected to active membership by separate ballot:

Dr. Charles L. Minor, Asheville.
Dr. William C. Rives, New York.
Dr. V. H. Norrie, New York.
Dr. Egbert Le Fevre, New York.
Dr. Arnold C. Klebs, Chicago.

The Auditing Committee reported that they had examined the Treasurer's books and found the accounts correct.

Having received an invitation to appoint a delegate to the International Congress for Tuberculosis and Public Health, to convene at Berlin at the end of May, 1899, Dr. Charles F. McGahan was appointed to represent the Association at the Congress, and was furnished with credentials.

The Chairman, Dr. Bowditch, and Drs. Otis and Peale, of the Committee on Health Resorts, made a report. The report was accepted.

Dr. Solly: I move that the officers of the Association take steps

whereby the Association can invite various authors and societies to send their publications upon climatology and allied subjects to the Secretary of the Association, and that the Secretary or chairman of such a committee as may have charge of this matter be instructed to keep a list of publications received, also a list of books so received and which are in the possession of the various members of the Association.

Dr. Otis seconded the motion. Carried.

Dr. McGahan suggested publishing such a list with the annual reports.

Dr. Solly said that it would be well to have such a list published every three or four months. That is, a list including the more recent works on these subjects.

The Secretary stated that we have had an exchange list in operation for some years past. We send out about seventy-five or eighty copies of our TRANSACTIONS each year to various societies and libraries at home and abroad, with the request "Please Exchange" on the wrapper. We have the British Balneological and Climatological Society on our list. The accessions to our library are not rapid enough to make the labor very hard.

Dr. Stubbert said he had received a communication from Dr. D. E. Salmon, the Chief of the United States Department of Agriculture, in which he calls attention to the activity of the anti-vivisectionists in this country; and in order to counteract their influence he intends to start a quarterly magazine to be called *Humanity and Science*, the object of which is to establish a regular means of communication between the vivisectionists. Dr. Stubbert said he had been requested by Dr. Salmon to bring this matter up before the Association. The anti-vivisectionists are becoming so powerful that they almost succeeded in preventing animal experimentation in the District of Columbia. If the doctors throughout the country would pay $1.00 per year for four years, it would pay for four copies of the journal, one to the subscriber and three to persons whom it would be wise to reach in connection with this subject.

Dr. Jacobi said he trusted the Association would give Mr. Salmon all the assistance he needs in that direction.

Dr. Knight stated that this Association has already taken action in this matter. He was chairman of a committee ap-

pointed for that purpose, and resolutions were drawn up and sent to the proper authorities in Washington.

After further discussion, it was decided to take no further official action in this matter, but that individually the members of the Association would do all in their power to aid the work of Dr. Salmon.

It was moved by Dr. Thomas D. Coleman, and seconded, that the thanks of the Association be extended to the New York members for the magnificent way in which the visiting members had been received and entertained. Carried.

It was moved by Dr. Knight, and seconded, that the thanks of the Association be tendered Dr. Robinson, the presiding officer, for his labors on behalf of the Association and for the effective and impartial manner in which he had conducted the duties of the Chair. Carried.

It was moved by Dr. Knight, and seconded, that thanks be returned to the Secretary, Dr. Hinsdale, and also to the Chairman of the Committee of Arrangements, Dr. Brannan, for their work in connection with the scientific and social features of the meeting. Carried.

It was moved by Dr. Knight, and seconded, that the thanks of the Association be tendered to the New York Academy of Medicine for generously placing at our disposal the hall in which the sessions were held; to Dr. J. Edward Stubbert and the Medical Board of the Loomis Sanitarium for their courtesies in connection with the trip to the Loomis Sanitarium, at Liberty; and to the New York, Ontario, and Western Railroad for their handsome provision of a special train to Liberty and return on the third day of the meeting. Carried.

The Council met at 2.30 P.M. A letter was read from Dr. Frank Fremont Smith inviting the Association to meet at Palm Beach, Florida. Inasmuch as the next meeting is to be held at Washington, the invitation was received favorably, and will be considered at the next annual meeting.

The Association was called to order at 3 P.M.

A paper by Dr. L. D. Judd, entitled "Remarks Based on a Further Experience with Calomel in Diphtheria," was read by title.

A paper by Dr. William M. Gibson, on "Embolism," was read by title.

Dr R. C. Newton read a paper on "Traumatic Rupture the Heart Without Penetration of the Chest Wall."

Dr. A. C. Getchell read a paper on "Bicycling in its Relation to Heart Disease."

Dr. Howard S. Anders read a paper on the "Relation of Local Meteorological Conditions to the Influenza Epidemic in Philadelphia in the Winter of 1898–1899."

Dr. Guy Hinsdale read a paper on the "Cold Wave of February, 1899."

Dr. Harold Williams read a paper on the "Effects of Violent and Prolonged Muscular Exercise Upon the Heart."

Dr. Thomas Darlington read a paper on "A Case of Aneurism of the Aorta."

The session was then adjourned, to meet in Washington, D. C., May 1, 1900.

The annual dinner was held at the Hotel Manhattan in the evening, and forty-eight members participated. The President, Dr. Beverley Robinson, acted as toast-master.

The President-elect, Dr. Jacobi, on being called upon, said in part:

You may wish to know the subject-matter of which I am going to talk in a more or less—I should say less—entertaining way. In order to know it myself I consulted Noah Webster. Books understand it all, almost as well as a junior hospital assistant after having enlightened his professors during the course of examinations. Noah Webster says: "Climate is the condition of a place in relation to the various phenomena of the atmosphere, as temperature, moisture, etc., especially as they affect animal life or man." I think the most conclusively elusive definition is contained in the "etc." That is what, probably, the farmer meant whom I found trying to wake up his drunken man. When I asked him what was the matter, he said the man did not seem to be in his right climate. The "etc." may mean a great many things, the most weighty interests of men included. We are, for instance, just now engaged in humanizing in another climate the semi-nude Asiatic islanders. If we cannot change or abolish their climate, we think we can at least change or abolish the aborigines.

But all this smells of politics, and should not belong here, for

doctors are expected to know nothing of hospitals, of their own country or of its concerns, or to care for them; they are expected only to study the advertisements of the wholesale drug manufacturers; perhaps a little anatomy, bacteriology and other ologies, but surely politics or statesmanship knows them not, and *vice versa*, as a rule. I have been told that there are but few of us who know the list of the Presidents, the year of the close of the civil war, the contents of the Fifteenth Amendment, the meaning of civil service, or the names of our own Senators, Congressmen, or home legislators. It is enough to know the names of Croker or Platt. That climate is profitable, for I do know that there are some men in town who got or recovered their places merely because they swore allegiance to Croker, or Tammany, or whatever you call the thing, after being thoroughly acclimated.

Webster says: "Temperature, moisture, etc."

Have we advanced much beyond what they knew or practised two thousand years ago? Hippocrates advised in protracted diseases agricultural occupation, that means air and exercise; Aretaeus, phthisical patients, sea air; Celsus, the sojourn in Alexandria, Egypt, with the notice that the climate which gave him a disease is the worst for the patient; Galen, high altitudes and milk diet; Pliny, the needle forests.

That was all there was of it, until W. von Humboldt studied the influence of climate on animals and plants; and modern pathologists, such as Mühry, made original observations on the actual conditions of populations compared with their climates; and until Sigmund, Charles Theodore Williams, and my life-long friend, Sir Hermann Weber, began to plough the million acres of our ignorance.

What we shall have to learn on climate is not a single fact, but a complex of facts, for climate is a composite of temperature, light, moisture, amount of rain, time of rain, direction and force of air currents, purity of air, electricity, ozone, and of the condition of the soil and of vegetation. In the same latitude, or even in the same district, the locality of the place to which you wish to send your patient should be known. Unfortunately, in the very same hotel, the healthfulness of the room a patient occupies may and does depend on the access of the sun, the exposure to light and wind, the proximity of ball-room, kitchen, or toilet closets. That is why the responsibility of sending patients to a certain place is

so very great, the circulars of hotel-keepers and the analyses of well-paid or well-meaning existing or non-existing chemists notwithstanding.

In such selections general principles may be guiding, but the constitution of the individual patient and his habits should also be considered. At all events, there are three main questions which should always be asked:

First. What is the influence of the climate on the indigenous population?

Second. What on healthy strangers?

Third. How have sick strangers been influenced by it?

What will climate do for our patients? Colorado, the South, the Southwest are constantly recommended as panaceas; so they are to some; so they are made to be hades to many. I meet a great many poor people with pulmonary diseases. There is not a day but I am made sick with the following report: "The doctor tells me I must go to Colorado immediately or lose my life." Who is it that has been told so by the all-wise doctor? A poor mechanic, mostly with a semi-starving family—a forlorn-looking tailor with not a ten dollar bill to his credit, with nobody to help him or to sustain his family. He is told by one of our profession—learned, enlightened, humane—that he is lost unless he goes to Colorado; to do what? to live in an attic, with a crowd of others, on poor food and as little as he can pay for, with distress for to-morrow and anxiety about his starving children left behind. What is the climate to do for him? And what is the common sense, humanity, foresight in the doctor who sends him away to starve and to pine and to die among strangers. He cannot get benefit from climate—as little as Tantalus had. Anxieties and sorrow eat men's souls in every climate; and in the purest of atmospheres men's blood is devoured by sleepless nights. As far as the better situated are concerned, we know what a change of climate can do for them. Unfortunately, a change of climate is often ordered too late, for many reasons; patients return unimproved or perish away from home. That is the principal reason why so many refuse to go when they are advised, even among the well-to-do classes. It is possible they will learn in future from the institutions established by communities or to be established for the poor. These, with their results, will be more impressive teachers than all the preaching of

the doctors to the better situated. It will be as it was with hospitals. The good results of the hospitals for the last twenty years have taught the household many lessons; so I think the sanitaria now existing and those planned will instruct the public at large in the lessons of climatology. They will have another incidental good result. Mutual dependence of all classes upon one another is now taught by the dangers of infections and contagions; the sick are daily learning that they and their children are constantly threatened and influenced by the sore-throat, erysipelas, whooping-cough, and scarlatina of their servants and their families whom they visit or by whom they are visited. They will also learn by judicious preventives—such as public sanitaria—that they will be preserved in their health through their aiding in the preserving that of the absolutely poor or the semi-dependent. If it be fear alone that leads the community to such preventive measures, the results will prove them to be profitable investments. In this way individual egotism and philosophical altruism meet for the accomplishment of the same results in the service of humanity and humanitarianism. After the teachings of socialism will have been, or are being practised, partly through necessity, partly through love, the name of socialism will cease to be a bugbear, and it will become evident why doctors are apt to be socialists.

Meanwhile, I should say that I know of one conclusive specimen, at least, of the blissful influence of climate. It is that of New York as long as the Climatological Society is meeting here; but then it is not the climate of New York, but that of the Climatological Society. As it agrees so perfectly with me, I drink to the permanence of the climate of the Climatological Society.

After responses from various members a toast was drank to the memory of Dr. Alfred L. Loomis, the first President of the Association.

On the following day a party of members and guests, to the number of seventy, left New York on a special train for Liberty, accompanied by Mr. J. C. Anderson, of the New York, Ontario, and Western Railway.

The party was conveyed in carriages to the Loomis Sanitarium, and entertained by the Medical Board at dinner. After some appropriate remarks by Dr. Beverley Robinson, a descrip-

tion of the Sanitarium and its work was given by Dr. J. Edward Stubbert, the physician in charge, and by Dr. Henry P. Loomis, of the Medical Board.

The members were then divided into groups and shown through the buildings, and the methods adopted for the treatment of tuberculosis were fully explained.

The natural advantages of the location—altitude 2300 feet—the perfect equipment and the success of the medical *regime*, made a strong impression on all who were privileged to take part in the programme for the third day of this memorable meeting.

GUY HINSDALE,
Secretary.

CONSTITUTION AND BY-LAWS.

CONSTITUTION.

Article I.—Name.

This Society shall be known as the American Climatological Association.

Article II.—Object.

The object of this Association shall be the study of *Climatology and Hydrology and of Diseases of the Respiratory and Circulatory Organs.*

Article III.—Membership.

Section 1.—This Association shall consist of *active, corresponding,* and *honorary* members, the latter not to exceed ten.

Sec. 2.—Names of candidates for active membership, whose applications have been indorsed by *two* (2) active members, shall be sent to the Secretary at least thirty (30) days before the annual meeting. On approval of the Council, the applicant shall be balloted for at the annual meeting. Three (3) black balls shall be sufficient to reject a candidate. The Council shall have power to nominate active members.

Sec. 3.—The power of nominating honorary and corresponding members shall be vested in the Council. The election shall be conducted in the same manner as that for active members. Honorary members shall enjoy all the privileges of active members, but shall not be allowed to hold any office or cast any vote.

Sec. 4.—Any member of the Association absent from the meetings, in person or by contributed paper, for three (3) con-

secutive years, without sufficient cause, may be dropped from the list of members by vote of the Council.

Article IV.—Officers.

Section 1.—The officers of this Association shall consist of a *President,* two *Vice-Presidents,* a *Secretary and Treasurer,* who, with five other members and the delegate and alternate to the Executive Committee of the Congress of American Physicians and Surgeons shall constitute the *Council* of the Association.

Sec. 2.—Nominations. The officers, including the Council, shall be nominated by a committee of five (5) members, which committee shall be nominated by the President at the first session of each annual meeting, and shall report at the business meeting.

Sec. 3.—Elections. The election of officers shall take place at the business meeting. A majority of votes cast shall constitute an election.

Sec. 4.—The President, Vice-Presidents, Secretary and Treasurer shall enter upon their duties at the close of the annual meeting at which they are elected, and shall hold office until the close of the next annual meeting, or until their successors are elected.

Sec. 5.—Members of the Council, other than the President, Vice-Presidents, Secretary and Treasurer, shall hold office for five (5) years.

Sec. 6.—Vacancies. Any vacancy occurring among the officers of the Association during the year may be filled by the Council.

Article V.—Duties of Officers.

President and Vice-Presidents.

The President and Vice-Presidents shall discharge the duties usually devolving upon such officers. The President shall be *ex-officio* Chairman of the Council.

Secretary and Treasurer.

As Secretary, he shall attend and keep a record of all the meetings of the Association and of the Council, of which latter

he shall be *ex-officio* Clerk. At each annual meeting he shall announce the names of all who have ceased to be members since the last report. He shall superintend the publication of the TRANSACTIONS, under the direction of the Council. He shall notify candidates of their election to membership. He shall send a preliminary notification of the annual meeting two (2) months previous thereto, and the programme for the annual meeting at least two (2) weeks previous to its assembly, to all the members of the Association. He shall also send notification of the meetings of the Council to the members thereof. At each annual meeting of the Association he shall read the minutes of the previous meeting and of all the meetings of the Council that have been held during the current year.

As Treasurer, he shall receive all moneys due, and pay all debts therewith. He shall render an account thereof at the annual meeting, at which time an auditing committee shall be appointed to report.

ARTICLE VI.—COUNCIL.

The Council shall meet as often as the interests of the Association may require.

Four (4) members shall constitute a quorum.

It shall have the management of the affairs of the Association, subject to the action of the Association at its annual meetings.

It shall consider the claims of candidates recommended to it for admission to membership.

It shall not have the power to make the Association liable for any debts exceeding in total one hundred dollars ($100), in the course of any one year, unless specially authorized by a vote of the Association.

It shall have the entire control of the publications of the Association, with the power to reject such papers or discussions as it may deem best.

It shall have power to nominate active members at the annual meeting.

The Council shall have power to invite any gentleman, not a member, to read a paper at the annual meeting, on any subject within the scope of the objects of this Association.

The Council shall determine questions by vote, or—if demanded—by ballot, the President having a casting vote.

The Council shall constitute a Board of Trial for all offences against the Constitution and By-Laws, or for unbecoming conduct, and shall have the sole power of moving the expulsion of any member.

The President, or any two members, may call a meeting, notice of which shall be transmitted to every member two (2) weeks previous to the meeting.

Article VII.—Papers.

Section 1.—The titles of all papers to be read at any annual meeting shall be forwarded to the Secretary not later than one (1) month before the first day of the meeting, in order to appear on the printed programme.

Sec. 2.—No paper shall be read before the Association which has already been printed or been read before another body.

Article VIII.—Quorum.

A quorum for business purposes shall be ten (10) members.

Article IX.—Amendments.

This Constitution may be amended by a four-fifths ($\frac{4}{5}$) vote of all the members present at an annual meeting, provided that notice of the proposed amendment has been printed in the notification of the meeting at which the vote is to be taken.

BY-LAWS.

1. Meetings of the Association shall be held annually.

2. The time and place of the meetings shall be determined by the Council.

3. The dues of active members shall consist of an annual assessment not to exceed five (85) dollars. Members in arrears shall not be entitled to vote. Those in arrears for two (2) years

may be dropped from membership by recommendation of the Council.

4. Order of business meeting.

First day:

Calling the roll of members;
Minutes of previous meeting;
Treasurer's report;
Appointment of auditing committee;
Appointment of nominating committee;
Report of Council on recommendations for membership.

Second day—Morning session:

Report of nominating committee;
Election of officers;
Election of members;
Report of the committee on health resorts;
Miscellaneous business;
Adjournment of business meeting.

Any of these By-Laws may be amended, repealed, or suspended by a two-thirds vote of the members present at any meeting.

PRESIDENT'S ADDRESS;

THE HIGH AIMS OF THE PHYSICIAN.

By BEVERLEY ROBINSON, M.D.,
NEW YORK.

The Climatological Association is no small fraternity of little men with no great end and object in view. Already by its work it holds an honorable position among other medical associations. Thanks to the tender care of those who watched and nurtured it at its inception—like Loomis and Pepper—continued and helped by Jacobi, Knight, Curtin, Walker, and others of equally pure metal, we have no reason to dread comparison with our fellows working in other fields. It is now well recognized wherever modern medicine is taught and valued that Americans are justly in the van. Where, indeed, do we find more earnest, more sacrificing workers? Are there brighter minds elsewhere? Does the world owe us much less than it does to the German or French? In one sense, perhaps, it does. We have not been able, it is true, owing to our lack of suitable endowments for laboratories and professors, to discover so many things which in their later application to the cure of disease, to the increased happiness and well being of the race, merit all our praise. And yet, are we not almost invariably disseminators and improvers upon what is indicated, as it were, by earlier work, and it may be more scientific?

Remark what important results we have obtained from the antitoxin of diphtheria and of tetanus; from tuberculin or

its modifications; indeed, from serotherapy in all the different forms; and last, but not least, from Listerism and all its splendid beholdings and outcome.

Alongside of what makes us feel so proud, so well satisfied, however, there is something which we would change if we could—which we shall change, God willing, in order that no reproach may be attached to our otherwise glorious record. In these latter days some men seem to love money too much. They appear to fall from their high estate in their contest for it. Not content with an honorable sufficiency, they want more wealth, and why? Is it to increase the sum of prosperity and happiness among those less favored than themselves? Is it to endow colleges, to promote learning, to ameliorate and lessen the burdens of the poor and downtrodden? Not always, I very much regret to say. They want money for money's sake, and that alone. Given everything almost that makes their lives honored and respected, they act as if the mere acquisition of lucre were their ultimate worldly desire. Do not mistake me! These are the few, the very few, and yet they hurt us; they hurt the noble ends we would all most gladly serve. Medicine, properly understood, is too high, too great a calling to be smirched by any such, and I stand here to-day to cry a halt and call upon the band who surround me, and who I know are with me, to bid them turn about and reform their ways. Unfortunately, here is the stumbling-block more particularly of the specialist. The general practitioner—the noblest of them all—the man whose life and service on the battlefield or in peace is "*sans peur et sans reprôche,*" who has been so often the stay and bulwark of the faint and weak, of the diseased, the wounded and the hopeless; *he* is not to be captured, as a rule, by any niggardly idea or brought low by any blot—though it be never so small—on a spotless escutcheon. And here it is, gentlemen, that we would speak, and that, too, in no uncertain tones of the *character* of the physician. Let him continue to be in the future, as he has been in the past, the saving help in time

of need of the sorrowful and sorrowing one; but let him also be, as he is now, and should be more and more, the guiding hand of the people in all matters relating to public health.

We want our cities and towns to be thoroughly cleansed, the sewerage to be carefully and properly attended to, our streets to be well lighted, our health board regulations to be governed with all scientific safeguards, looking to the common good, and alike for the poor as well as for the rich. It is for us to insist upon, every day and at all times, the evils of our tenement-house system and the moral turpitude and physical debasement which necessarily proceed therefrom. Of course, I recognize that broad-minded, generous, noble citizens within and without our profession have already come to the front and have given their work, their time, their money, their influence, to change these canker sores of our Greater New York, of our Empire City. On the other hand, political greed and ambition have stood in the way of a grander, larger, more extensive, and far-reaching work. The day will come, however, and none too soon, when with our electrical railways and through trains on every city line, our numerous bridges over both North and East Rivers, the man of small means will be able to give his wife and family and self the contentment and joy of a real home, where peace and moral worth and elevation of thought and action dwell continuously.

In the family relations, also, the chosen doctor who is to guide and direct will be selected not because he knows everything pertaining to medical and surgical science, but because *he* among other men knows the *most*. If there be, as there must frequenly be, occasions when the specialist's advice is sought for and required, he will stand by as the guardian of the family's first interests; he will be requested not to allow any narrow views to control the situation; not to permit any interference, medical or surgical, to be carried out unless with his entire approbation and indorsement. And the patients

will select their physician because of his well-known, well-grounded intelligence, his previous long and honorable record of good and faithful work accomplished before his fellows and in the light of day—knowing as they will and must that he does for them not merely as he does for himself, but rather as he does for those he loves best in the world—heart within and God o'erhead. To repeat once again, for the physician as for other men, the noblest aim of his ambition should be to establish *character*. To lose it is to be, as it is related of a renowned French orator:[1] "When Mirabeau, in the consciousness of the possession of the most masterful genius of his time, rose to speak in the National Assembly, he became aware that his dissolute past was standing beside him and mocking him. His vast power, honestly put forth for great ends, was neutralized by a record which made belief in him almost impossible. In bitterness of soul he learned that *genius* and *character* are bound together by indissoluble ties, and that *genius without character* is like oil that blazes up and dies down about a shattered lamp."

On the other hand, to gain it and firmly establish it, it is to thoroughly fill the soul's best longing. Then he may become ideally, at least, such a one as Dr. Butler, "Master of Trinity," describes Gladstone in the finest memorial address which his death called forth:

"Critics, friendly and unfriendly, might weigh the varied elements of his rare intellectual structure; its range, its subtlety, its mastery over men; but the verdict of multitudes and of nations has been rather this: He gave us many gifts; but the most precious and the most enduring was *himself, his character*. He lived and labored with God before his eyes. He had the fear of God before him, and made a conscience of what he did. He loved righteousness and hated iniquity. His heart was with the poor and the wronged and the down-trodden, and dear was their blood in his sight. It is this

[1] The Outlook, September 10, 1898, p. 133.

conviction which at this hour draws us 'with the cords of a man' and makes us all of one mind."[1]

In Joseph H. Choate's splendid oration, delivered by him at the unveiling of the statue of Rufus Choate, the great lawyer and advocate, in the new court-house of Boston, the speaker said: "And first, and far above his splendid talents and his triumphant eloquence, I would place the character of the man—pure, honest, delivered absolutely from all the temptations of sordid and mercenary things, aspiring daily to what was higher and better, loathing all that was vulgar and of low repute, simple as a child, and tender and sympathetic as a woman. So let the statue stand as notice to all who seek to enter here that the first requisite of all true renown in our noble profession—renown not for a day or a life only, but for generations—is *character*."[2]

In the Harveian oration on "The Influence of Character and Right Judgment in Medicine,"[3] delivered before the Royal College of Physicians of London, on October 18, 1898, by Sir Dyce Duckworth, the speaker said:

"We are perhaps too much disposed to commemorate the scientific achievements of our great men, but let us not be unmindful of their characters. We know that genius is not always coincident with the highest moral or spiritual perfection, but when both these qualities are graciously combined in any one we feel that we are in the presence of a truly great man—of one who becomes a personage and a power for good in his day and generation. In such a profession as ours we can never afford to lose sight of the preponderating influence of character in all who join our ranks and have to minister to every grade of our common humanity."

And further on says Dr. Duckworth in eulogizing the immortal Harvey: "The day in which it has been affirmed that there is a gulf fixed between physical and spiritual science is, I think and hope, fast drawing to a close."

[1] E. L. G. in the Evening Post. [2] New York Tribune, October 15, 1898.
[3] The Lancet, October 22, 1898, pp. 1037–1043.

As Willis observes, Harvey "seized every opportunity of giving utterance to his sense of the immediate agency of the Divine in Nature." He had no fear either of the processes or the results of research. We can imagine his approval of the following sentences in the *Religio Medici*, which he must have read: "There is no danger to propound these mysteries, no sanctum sanctorum in philosophy. The world was made to be inhabited by beasts, but studied and contemplated by man; 'tis the debt of our reason we owe unto God and the homage we pay for not being beasts. . . . . Those highly magnify Him whose judicious inquiry into His acts, and deliberate research into his creatures, return the duty of a devout and learned admiration. Therefore:

> "Search where thou wilt; and let thy reason go,
> To ransom truth, e'en to the abyss below;
> Rally the scattered causes; and that line
> Which Nature twists be able to untwine.
> It is thy Maker's will; for unto none,
> But unto reason can He e'er be known."

Again speaks Duckworth: "Harvey himself best summed up his great characteristic and guiding principle in a concise sentence, which might even now be fitly inscribed on his sarcophagus, and it is this: 'I avow myself the partisan of truth alone.'"

That was the great moral of his life—truth eternal, ever to be sought for, to be held, and to be passed on.

To some men, perhaps, in what I have tried to outline, they may find things to criticise or take exception to; they may say all this is very well, but is it not Utopian, impracticable, impossible? I presume this is true, but it is equally true of all idealism, of all perfection. And yet what we very much need to-day in our lives is this very idealism. We have enough, and more than enough, of the *practical*. Our daily routine is eminently practical. Most of us are obliged to work, to labor, to save. We know that in this

way, and this way alone for the majority of men, success is ultimately reached. Ignorance and deception and false ideas of life and its purpose surround us. Worldly applause, the desire to have fame at any price, and even though it be acquired by means that are unworthy; the constant, ceaseless endeavor to outstrip one's fellows and get ahead, may capture those who are filled with a narrow, selfish ambition. But is such a struggle to be imitated or admired? Is the reputation thus acquired ever enduring? I am sure it is not. The men whose fame is perennial, whose deeds are praised and glorified after they have gone and disappeared from sight, are those whose memories give us always a thrill of enthusiasm which awakens what is best within us and tells of a goal to win that does not pass away and is not purely chimerical.

With the rapid growth of specialism, with the constant search for new things, with the ever-increasing scientific investigations in so many directions, with the augmented opportunities and necessities of chemical, microscopical, and bacteriological findings, the older practitioner is apt to think that his previous education and training have become time-worn and obsolete. But is it so? Should we not insist and believe that they are all the more valuable. Medicine is no art of yesterday, nor is its science altogether modern. The ancients have handed us down much, very much, that is still true, that always will be true. We, in our day and time, have added to the store of valuable knowledge. And this, too, must remain. Many things we once thought were good and would endure have been found useless, and hence are now neglected or forgotten; but so it is, or will be, with much that is now highly thought of and esteemed. With this conviction is produced a wise conservatism of thought and action. The older practitioner is conservative, and rightly so, not because he dreads new discoveries; not because he would stand in the way of legitimate progress, but simply because he knows that "all is not gold that

glitters," and that shams and frauds are many; that enthusiasm needs to be controlled; that knowledge does not come in a day, and that the sifting of the wheat from the chaff is always a labor of time and careful, prolonged, continuous inquiry and repeated observation. Reverence for men and things of the past must be kept alive, not followed and adhered to with the praise and laudation of a fetich; but, nevertheless, not derided and abandoned altogether, because other things are newer and, consequently, to many minds, more attractive.

How often do we hear the reproach made to the older practitioner that he has become antiquated; that he is no longer abreast of the times; that the younger man must know more because he is fresh from the schools, and hence he has the latest and best equipment! But is this often true? Is not the knowledge bought of and through experience something that experience alone can give and justify? How many prescriptions are uselessly written and unwisely taken that come from the junior members of our profession! How many surgical operations are performed that may add to the budding *éclat* of our brilliant young operator, and yet if he could but see and thoroughly understand how small, if any, is the ultimate advantage for the patient to be derived from them, would not his hand be withheld? Would he not be content to advance more slowly but more surely, and certainly more conscientiously?

Happily for us to-day, medical education is higher in its intellectual grasp than it was a decade or two ago. A longer, more arduous ordeal is of necessity gone through to obtain the right practice, and for this reason errors of doing because of mere ignorance, do not occur so often. The people who swallow nostrums, who are cured by friends, acquaintances, or over the drug counter, are not, as a rule, the educated, trained men and women. Fortunately, no doubt, the too-confiding victims are the silly and the weak, the ignorant and thoughtless, those who imagine that knowledge and

information come by sleight-of-hand or sudden inspiration. The best of us know absolutely the contrary. To these misguided and foolish ones I would point unerringly to the myriad of patient, laborious workers whose life and deeds blazen forth the one truism of constant unremitting toil. *Toil* not for bread, not for fame, not for worldly rank, not for popular applause, not for greed, but simply, as de Tocqueville writes, for *duty to be done and well done.* Faraday, Huxley, Tyndall, Pasteur, Humboldt, Lister, Virchow, Sanderson, Bastian, and countless others "who spurned delights and lived laborious days," prove it and glorify it.

One of the retorts which the specialist sometimes makes to the general practitioner is that without his help the patient would not, could not, recover. He firmly believes it is his medical or surgical skill which gets the success in curing the sufferer which would not otherwise be attained. Whenever we say to such a one, "Friend, the body of man is not perfect, physically or functionally, as a rule—and assuredly this statement is correct after middle life is entered upon—therefore it is not wisdom beyond a certain point to try to reach a standard that we shall not probably secure; indeed, that is scarcely possible, unless the personal sacrifice be very great, or other conditions are made morbid, or hurt, through our foolish efforts," there is no responsive answer. The latter, however, is very frequently the attitude of the general practitioner, who, with his broad appreciation of all bodily ailments, knows when to say halt, when or how to interfere. It is not always given to him to be able to do just what should be done, if it require special skill or training to do it well; but he always should be the guiding hand, the referee, to whom any grave or even apparently small matter is taken, and by whom ultimately it is decided. Of course, in the process of time and with increased success in certain lines of practice, men as they grow older—and particularly the better informed and more skillful and capable ones—will gradually drift, as it were, where some chosen field of work

lies. But here is a sort of natural law which governs and which ultimatly produces the best results. But this is very different, and far better than the plan which tells a man that he should do this or that sort of work before he can possibly know what he is best fitted for, and without the long preliminary training which makes him a worthy applicant for ultimate and legitimate great success.

One great result, if no other, is assuredly achieved. Medical men do not see at every turn in the road some slight local defect which they wish to remedy with mere local means. They become broader and larger. They note the interdependence of all our organs, of all our functions; they generalize, as they should, and from numerous isolated facts grasp mighty laws which govern. This is the correlation which almost invariably exists in our bodies sick or well. It is for us to recognize it and follow it fully and rationally wherever it leads us.

Anatomy, normal and morbid, teaches us many things; but to see in mere structural conditions or changes everything, and not allow for the wide influence and omnipresent functional disturbance, is surely to narrow very much our power for good. How often do we go to the autopsy-room to find multiplied diseased conditions of organs, and yet when the history of the case is carefully analyzed, are we not astonished that with such, if they really have primary importance, all previous health or enjoyment would have been wholly impossible? On the other hand, do we not frequently see the autopsy made with greatest care without detecting any obvious diseased condition? Nevertheless, the patient's life has been one long series of disablements and sufferings, and to crown them all death comes. The vital force, the latent energy, the dynamic, essential thing is also the unknown and probably always will be.

The great physician of to-day should be the man who thus sees and reads Nature. He should be the one, of course, who studies the influences of climate and soil; of environment; of habits and customs; of professional and business lives; of

light and heat; of winds and dust and dirt; of moisture and dryness, and all such influences for health and disease. But into his mental appreciations should also be the idea of thought and feeling and emotion; of cares and sorrows; and when he thus becomes, can he ever thoroughly abrogate the true missionary spirit? Is not the physician the one who has got to have it in the mere nature of things? Is he not obliged to minister to the soul as well as to the body? I do not mean or intend to convey that the physician is to teach men any of the things which pertain merely to sect, or to try at any time to clothe himself with a priestly mantle. I do mean, however, that by patience and symptathy, forbearance and long suffering and absolute rectitude in all he does and all he hopes to attain, he should be the highest ideal it may be given on earth for a man to follow. If such be the man, outside and beyond his purely professional calling, the great and good physician becomes the great and good citizen. His civic virtues are many; he obeys law and order; he points the way to a higher and nobler moral sense; he works not for hire, but for all men's best good. He points and directs the way in all social reforms. He is but a reflex in the body politic of everything that men should hold righteous and of good report. Immorality has no place near him; indecency of thought and word should not be countenanced by him. Reverence for Nature—Nature's laws and the over-ruling God—the moving, essential spirit of all law in this world and in the next are his guiding star. Self-sacrifice for men which he inculcates beyond any price in his own life and example becomes then, and always must and should, what leads men more than anything else to recognize the brotherhood of men and the saving help of Him who died to make men holy.

Having endeavored to show briefly how essential it is for the physician to establish character; how important it is also for him to educate his heart and mind so that he may ever be in close touch of sympathy and affections with his patients;

how true it is that he should never lose sight of the fact that he, too, in his person must ennoble, if possible, all civic virtues ; and how, finally and above all, he must inculcate by his example and doctrine the purest aspects of Christian living, I still have a word to add for the scientific and practical side of medicine—their relations and interdependence. To-day we think a great deal of science—its advancement, its wonderful conquests. We feel as though things to be true must be proved, and in matter of proof we are apt to abide too closely by the results of mere laboratory research; and even before the test of time has thoroughly shown their possible falsity or shortcomings, to allow ourselves to be guided and directed by them. Everywhere and at all times nowadays we hear and read of the omnipresent microbe and of his fearful ravages. Seldom do we hearken to the fact that he is often a very innocent or inoffensive factor in our bodily ailments. Whenever we have given him a form, size, aspect, immediately we infer that he always represents trouble and disaster. At least such is the attitude too frequently of the *profanum vulgus ;* and likewise, let it be candidly admitted, of the not overwise scientist, who forgets that the art of medicine is the practice thereof, and that this art is quite as jealous a mistress and needs to be served with just as much fidelity and patience and brains as the science upon which the art is in part based.

In many diseases of an infectious type[1] the rôle played by bacteria, it seems to me, has been misinterpreted. They, in themselves, are not the causative agent of these maladies, but are rather a frequent accompaniment of the septic process, and may act as mere carriers of contagion. It is not difficult to understand, therefore, that when once the septic action is begun by the presence and influence of a poisonous principle of the contgious or infectious disease, such change may take place in the liquids and tissues of the body as to promote the

[1] American Journal of Obstétrics, 1876, vol. ix. p. 258 et seq. Robinson : " Nature of Diphtheritic Poison, and Its Treatment by So-called Disinfectants."

rapid production and development of different forms of bacteria, with more or less characteristic features.

And after this manner may be satisfactorily explained the almost indefinite production or reproduction of the septic poison in any one of the diseases referred to. Let us beware, however, of confounding the really pernicious substance with an agent whose influence, to say the least, is at times probably but indirect and secondary. Once more, the bacterium is the abode of the poison, not the posion itself. This dwelling-place, as it were, may remain the same for quite a period of time and through an uninterrupted succession of many different individuals. It may also be destroyed or changed by the use of methods which occasion separation of the poison from the microbe, or else which destroy the virulent principle by decomposing it, or simply neutralizing its pathological action.

There are experiments to show that salicylic acid and carbolic acid, while excellent preservatives and antimicrophytics, are perhaps not always disinfectants in the strict sense of the term. In other words, the true poison of virulent disease will remain unaltered by their action even though lower organisms are destroyed and the power taken away by which their reproduction is effected.

If this be true of the noxa of virulent disease it is probably true also of the poisonous principles of zymotic affections in general. In these diseases, therefore, when by the most perfect and thorough employment of antiseptics we have taken it for granted that we have destroyed their special cause of transmissibility, we have possibly tended rather to preserve it intact than to annihilate it. And what applies to the acids mentioned above, as types of a large class of so-called disinfectants, may apply equally well to many other substances of different chemical and physical properties, but which are co-ordinated by many physicians with the former on account of a similar destructive action which they exercise over lower forms of life.

It behooves us, in this as in other difficult or problematical subjects of medicine, to liberate ourselves from preconceived or too absolute theories which impede our progressive march in the path of exact knowledge. The following conclusions taken from the address of Charlton Bastian, in which he attempts to refute the too exclusive views of the upholders of the germ theory of zymotic diseases, contain even at the present day some wholesome truths:

"1. The virus, or contagium of some of these diseases, whatever it may be, does not exhibit the properties of living matter.

"2. There is the extreme improbability of the supposition that this whole class of diseases should be caused by organisms known only by their effects.

"3. The facts of the sudden cessation, periodical visitation, and many of the other phenomena of epidemics, however difficult they may be to explain upon any hypothesis, seem to oppose almost insuperable obstacles to the belief that living organisms are the causes of such epidemics of specific contagious diseases."[1]

To Bastian's conclusions, promulgated twenty-four years ago, I would now add citations from the Huxley lecture on "Recent Advances in Science," by Professor Rudolph Virchow, delivered last autumn in London on a memorable occasion: "Virchow writes:[2] "With the discovery of parasitic animals conjecture became fact, and nothing was easier than to generalize this fact and assume the presence of independent organisms in each contagious disease. . . . It may be said that the contagious nature of disease shows suspicious of bacterial origin, but it should not be simply called that, athough it allows the conscience to sleep. Some of the most important contagious diseases have succeeded in resisting the struggle to find in them a parasitic contagion. For example, many have been the sanguine hopes of finding the parasite of

[1] Delivered before the London Pathological Society, April 6, 1875.
[2] The Lancet, October 8, 1898, p. 911.

syphilis, and as many have been the failures; the coccus of gonorrhœa alone has been discovered, the bacterium of syphilis remains a desideratum. You will remember the certainty with which it was expected that a parasite was the causal agent of variola—more than one was found, but none pathogenic. In hydrophobia all appearance seemed to promise that it would prove to be a microparasitic disease. Its contagion is undoubted; the vaccine has been prepared, and yet no one has been able to cultivate a specific bacillus. The same may be said of some other specific diseases. Painful as it may be, one can do nothing but observe. Perhaps the pathogenic bacteria will be found, but as long as they are not discovered all assumption is useless, if not dangerous. To have learned this is to have made a mighty stride in the biology of disease."

"It was a happy inspiration," writes the editor of the *Lancet*, "which led the authorities of Charing Cross Hospital to invite Professor Virchow to deliver the Huxley lecture on the occasion of the opening of the medical session.[1]

"They could not have better honored the name of the eminent man whose medical education was obtained in their institution than by linking it in this manner with that of the foremost living exponent of pathological science. . . .

"Perhaps the most interesting portion of this lecture was the exposition of parasitism and the great extension which has been given to this in the discovery of the minute parasitic agents which excite the infectious diseases. Professor Virchow did well to remind us that the reaction of the tissues and organs to the bacterial poisons forms an essential part of these diseases, and he, perhaps wisely, bade us remember that there may be some infections in which bacteria have no share."

In corroboration of, as well in advance, in my judgment, of Professor Virchow's ideas, I would cite from a paper read by Professor Martins, of Rostock, at the annual meeting of

[1] The Lancet, October 8, 1898, p. 940.

the German Naturalists and Physicians at Düsseldorf[1] on September 19, 1898, on "The Cause of and Predisposing Conditions for Disease."

Professor Martins says: "What predisposition or tendency of the organism gives the opportunity for the vegetable parasite to gain a foothold we are no nearer knowing than before, yet this is *the* important element in the origin of disease.

"There is a time in the inception of modern pathological anaomy when the discovery of the disinctive pathological lesion of the disease was thought to have brought medicine nearer to *ens morbi*, to the essence of the disease. When it was found, for example, that hepatization of the lungs was chacteristic of pneumonia, it was considered that a distinct step had been made toward knowing the entity of inflammation of the lungs. Now we know the bacterial cause that sets up the hepatization, but we are no nearer knowing why one man is afflicted with it, another not, although both seem to have been exposed to similar conditions and in the mouths of both pneumococcus may be demonstrable. . . .

"If exposure to microbic infection was always followed by disease, then bacteriology would have given us an explanation of the origin of the disease. Infection and disease are by no means correlative terms, and the difficult question of susceptibility comes in as an important element to make us realize that we are still a long way from the explanation of specific pathogenesis. The study of susceptibility and its causes is to be an important occupation for the pathologist of the twentieth century, and much more is to be hoped from a widening of our knowledge of predisposing conditions to disease than from the further discovery of specific bacterial causes. This advance of knowledge, however, will come from no one-sided bateriological hygienist, but from those who have studied profoundly the conditions of disease as they are."

Let us, then, erect our laboratories, promote scientific research, be wisely liberal and generous to those who with

[1] Medical News, October 22, 1898, pp. 539 and 540.

microscope and reagent and vivisection set the pace in a measure for the clinician. Let us not, however, ignore the claims of the latter to our fealty and lasting trust when mere science will not avail us much. I cannot do better here than to quote from an address delivered by Professor William H. Draper before the Association of American Physicians.[2]

"We must pursue science," says Dr. Draper, "so as not to imperil the progress of art, and we mush cultivate art as if science were not so much its mistress as its handmaid.

"We may wisely recall and apply the answer which Iphicrates, the Athenian general, is said to have made when he was hard pressed by an orator before the people, to say what he was to be so proud: 'Are you a soldier, a captain, an engineer, a spy, a pioneer, a sapper, or a miner?' 'No,' said Iphicrates, 'I am none of these, but I command them all.'

"So the practical physician may not be a very learned pathologist, an erudite physiologist, a skillful chemist, or an expert bacteriologist, but he must command them all; and it is he who must finally, through the co-operation of all the sciences which they represent, perfect the most beneficent of all arts; it is he who in his high mission as the servant of humanity must attain that wisdom which results from combining knowledge with the instinct and the skill for its useful application."

In a previous portion of this address I quoted from Dr. Butler's memorial tribute to Gladstone to show the proper estimate made in glowing terms of that great man's personal character. I shall terminate my remarks to you in, I believe, a most fitting manner by quoting from Mr. Gladstone's tribute to a great and noble physician, Sir Andrew Clark. At a meeting held in London to take definite action upon his memorial, Mr. Gladstone paid a most eloquent tribute to his late physician. He said:

"It appears to me that it was eminently desirable that, in a time like this, a man such as Sir Andrew Clark should rise

[2] Transactions, vol. iii. pp. 7 and 8.

to the head of his profession; for, after all, we require something more than knowledge, something more than skill. We require great devotion to the purpose of the profession; and that devotion I think, was never exemplified in a more remarkable manner than in the career of Sir Andrew Clark. He loved his profession with his whole heart and soul. While engaged in that profession he loved it not only with sincere and cordial, but with chivalrous devotion. We need not say that the age of chivalry is altogether passed so long as we have among us men of the type of Sir Andrew Clark.

"I think the profession has done well in taking by common consent Sir Andrew Clark as the typical man, the representative of all that is best and noblest in the profession and its purposes. Others may judge better than I can of his scientific ability. What I have seen in his patience, his thoroughness, and his absorption in the care of his patient as if that one case was all with which he had to occupy his mind. I have had to note in my own instance a warmth of friendship and assiduous prosecution of the task of watching my health which I know not how adequately to describe. Although he was a much younger man than I, yet he followed me from month to month, and week to week, with something that resembled paternal affection. I am sure that whatever happens; whatever may have been the past advances of the medical profession, and they are great; whatever may be the future advances of that profession, and they will be greater still—there will never come a time when the profession will not be justly satisfied and glad to have recorded upon its annals a name such as the name of Sir Andrew Clark." [1] May it be the immortal privilege of one of us to have such a eulogium from such a man!

"*Sic itur ad astra.*"

[1] Boston Medical and Surgical Journal, June 7, 1894, p. 579.

LIGHT AND AIR IN THE TREATMENT OF CONSUMPTION IN COLORADO.

By CHARLES FOX GARDINER, M.D.,
COLORADO SPRINGS, COLO.

In the etiology of pulmonary tuberculosis one fact stands out more clearly and is proved more positively than any other; that is, the enormous influence such simple factors as light and fresh air have upon the development of this disease. In fact, from the dawn of human history this influence has been noted,[1] and all human experience as well as scientific research have demonstrated that consumption is a disease of the indoor domestic animal, man included, and that when air and light are curtailed then consumption begins to strike down those who from deficient lung power are least fitted to survive. Another fact now proved beyond question is that, reversing the greatest predisposing cause (a lack of air and light), and supplying the air and light to excess, we have the most powerful weapon now known with which to fight and conquer this wide-spread and destructive disease. Even nutrition, important and powerful as it certainly is in treating the tubercular, must hold second place to light and air. I have seen hundreds of range cattle exposed to air and light dying of starvation and cold, and never a case of tuberculosis among them; while, on the other hand, cattle from the same herd, sheltered, well fed, and cared for, do occasionally develop the disease. It seems to me that in the face of such strong evidence as we can now show as to the value of air and light in curing consumption (a remedy far more effectual

[1] Aryteus Celus: Treatment of Consumption. Harris and Beal, p 4.

than all other agents taken together), the average physician passes over them with scant notice, and the brief order to the patient, "Be out all you can," results in but a small daily dose of fresh air—a quantity entirely too limited for the result desired. That this is a mistake, and that the most efficient remedy we have for consumption should be used with care, system, and in heroic doses, is very clearly shown by the results obtained in the sanitoriums both in this country and abroad.[1]

When used as a cure the qualities of the air should be considered; these depend upon climatic conditions,[2] and to state, as some men have, that conditions being equal in other respects, climate is of little importance, is not true or logical. As the dose and purity of a drug determine its favorable action upon the diseased condition it is intended to cure, so the dose and purity of the air and light, used as a cure, determine their efficient action in consumption. Any student of climatology knows that air and light differ materially in available dose and purity, even in narrow geographical limits, while a more extended view shows us climatic changes much more intense and radical. Air ought to be (1) pure, free from dust and germs;[3] (2) dry and thin, to favor chest ex-

[1] "Hospitals and Sanatoria for Consumption Abroad." Edward O. Otis, M.D., Boston Medical and Surgical Journal, 1898, p. 22.

[2] Braithwaite's Retrospect of Medicine, January, 1899, p. 236.

[3] "Tuberculosis and Altitude." In a recent work published in Mexico (La vie sur les hauts plateaux), which won the Hodgkins, prize of the Smithsonian Institute, Herrera and Lope devote a chapter to the treatment of tuberculosis by altitude, noteworthy in many respects. They find that statistics show that not only in men but in lower animals tuberculosis is decreased in high regions. In 1885 out of 73,000 cattle killed in the general abattoir of the City of Mexico, only forty-five were tuberculous. This favorable effect they attribute to the higher solar illumination in high altitudes and the dryness and coolness of the atmosphere as working against the existence of microbes. The benefits of rarefied air in consumption are given by the authors from their experimental and other observations, as follows: 1. Lessening pressure increases the circulation of air in the lungs, dilates them, and obliges torpid parts to functionate. 2. Lessening pressure determines a greater quantity of blood in the lungs. 3. Lessening pressure permits a uniform distribution of blood, regulates its circulation and combats congestion. 4. Lessening pressure diminishes intrapulmonary tension in general and particularly intravascular tension. 5. Augmentation of red and white globules. 6. Desiccation of mucous surfaces; the favoring of evaporation, besides experiments on animals, observations of actual cases of tuberculosis treated by rarefied air are reported by the authors. Of 13 cases thus treated only 1 lost weight, 1 remained stationary and 11 notably increased, 1 increasing 300

pansion, to favor greater supply of blood to the lungs and increased richness to the blood, and also to permit greater penetration of solar rays, thus limiting tubercular infection from others;[1] (3) cool, to act as a stimulant; and (4) with an excess of ozone and electricity.

Light should be sunshine, or directly diffused from it, and with as little cloudy or white light as possible. With these qualities of light and air we have at our command the most powerful agents known for the cure or arrest of pulmonary phthisis, and these perfect atmospheric conditions are found only upon the dry, high plateaux of the world. Some of the qualities spoken of are found at the seashore, some in low elevations, as in a desert atmosphere; but for all the qualities mentioned in both light and air altitude and dryness must be present. The effects of sunlight upon germ-life are well known to all,[2] and recent observations have shown that a partial absence of light tends to and even does develop consumption, even when the air-supply and other conditions remain the same.[3] Then, too, the direct solar rays are now known to penetrate live tissue and exert their influence as germicides. Experiments by Koch, Downs, Blunt, Duclaux, Esmarch, and Arloing are cited by Abrams—all tending to show the power and penetration of solar rays, their effect as a stimulant of cellular life of plants and animals in health and disease, and their power as a cure.[4] There is also good reason to believe that the solar rays pass through ordinary clothing and have a stimulating effect upon capillary circulation and nerve-endings.

A climate which has all the qualities of light and air that

grammes in one day. In none, either healthy or tuberculous, were the alarming symptoms described by Paul Bert experienced.

"Infected Atmosphere," by Guy Hinsdale. Medical News, May 21, 1896. Dust and Its Dangers, by T. Mitchell Prudden, M.D., pp. 28 and 29.

[1] Gardiner. American Journal of the Medical Sciences, 1892 and 1897.

[2] See note 1, p. 2 of this paper.

[3] "Climatology of Nudity." Transactions of the American Climatological Association, 1898, p. 233. Dr. William D. Robinson. Hygienic Prevention of Consumption. Edward Squires, M.D., p. 125. The Prevention of Consumption, by Morrill, p. 61. Pulmonary Tuberculosis: A Report of the New York Board of Health.

[4] Researches on Tuberculosis. Arthur Ransome, M. D. 1897. Page 71.

I have mentioned ought to present ideal climatic conditions for the cure of consumption. In Colorado these climatic conditions do exist. Not only is there an average of 50 per cent. more sunshine every day in Colorado that in any State east of the Mississippi River,[1] but the soil, temperature, and dryness of the air permit the pulmonary invalid to take advantage of these conditions. A simple experiment illustrates atmospheric light penetration in Colorado: Expose several films in a camera (time exposure), and then, under the same conditions in regard to films, time, sunlight, etc., expose the other half of the films at sea-level or at the seashore, and the latter set of films will be found to be fogged from under exposure, the more dense air not allowing light to penetrate as easily as in Colorado. It is probably this property in the air of Colorado, dermithisity or thinness, that removes color so rapidly from all dyed fabrics, tans the exposed skin so easily, and allows the sun and diffused light to act so powerfully as a germicide.[2]

The practical point is to utilize this light and air in the best possible way for our patients in Colorado, or in all sections of the country having these climatic advantages.[3]

To ascertain the time spent out of doors by the average invalid in Colorado Springs, I have departed somewhat from the method of making meteorological tables, which show merely the number of days suitable for him to be out, and have compiled a list giving the time an average invalid actually spends in the open air. Meteorological tables, no matter

[1] Colorado Springs Region as a Health Resort, pp. 6 and 7, by C. F. Gardiner, M.D., and Gilbert McClurg, Secretary, 1898. Colorado, by J. C. Dana and Carroll E. Edson, M.D., Denver, 1898, page 2. "Therapeutic Value of the Solar Rays," by Albert Abrams, M.D., in the Philadelphia Monthly Medical Journal, March, 1899.

[2] Gardiner. "Tent Life for Invalids in Colorado," in Climate, November, 1898. "Hygienic Treatment of Consumption," by Halbrook, p. 203. "The Alpine Winter Cure," Weir, Harris, and Beal, Treatment of Consumption, p. 83. "The Disinfection of Tubercular Infected Houses," Ransom and Deleptue, British Medical Journal, February, 1895.

[3] "Observations upon Pulmonary Tuberculosis in Colorado," by S. G. Bowney, A.M., M.D., Boston Medical and Surgical Journal, September 16, 1897. "Pulmonary Tuberculosis," by Edward A. Otis, M.D., Boston Medical and Surgical Journal, 1898. "Essentials in the Treatment of Tuberculosis," by Edward J. Birmingham, A.M., M.D., Philadelphia Medical Journal, March 18, 1899.

how carefully compiled, give but a limited idea as to certain climatic details. Temperature is frequently misleading, as in Colorado the dry air allows an outdoor life at temperatures that would in a more humid climate be impossible or dangerous, while such climatic factors as dust are not noted in ordinary tables, and so on. This chart has been compiled from an average of observations taken during the time from December 16, 1898, to April 19, 1899. This was to make as severe a test as possible, since it is during these months we have in Colorado our most inclement weather. As last winter and spring were of unusual severity; in fact from all I can learn all told the worst weather for any length of time of which we have a record, one can see that in this chart no flattering picture is drawn regarding our winter weather. By average invalid I mean those who are too ill to exercise by horseback riding, cycling, or golfing, or even walking, except very slowly, and yet are able to be up and about and to spend considerable time out of doors, provided they are protected and can sit or lie down. The time spent out doors can be regulated to a fine point in a sanitorium, but I refer to invalids who are in their own homes, in hotels or boarding-houses. I tell these patients, as most doctors do, that they must be out all they possibly can. I specify the hours and arrange the details of shelter, etc., selecting the winter months, as that is the season when an open-air life becomes so necessary to the pulmonary invalid, for he feels the cold, and will instinctively choose the warm and often close air of a room, inhaling furnace gas, dust, and germs. In summer it is, on the contrary, an easy problem to induce patients to live an out-door life. Then all nature beckons to one, and even indoors the air is fairly pure by comparison, owing to the open doors and windows and the absence of artificial heat. I find that the average invalid spends outdoors daily, either on a piazza, driving, or at times walking a little, about five hours. If the air and light are as important curative agents as I have stated, then five hours daily outdoors are not enough. We would aim at twenty-four hours. If we desire

the best results we cannot afford to waste a single hour in fighting such a disease as tuberculosis. Few people realize the vast difference that exists between a so-called well ventilated room and the open air—the former is enough to kill an Indian.[1]

The experience of one who has just returned to his ordinary habits from a life of several weeks spent entirely in the open air illustrates this. No matter how well ventilated the room in which he sleeps, it will seem close, and a cold in the head is apt to result. A simple experiment seems to me to bear closely on this subject. Take a piece of fresh venison or any meat, and hang half of it in a room protected by fly screens, hanging the other half on top of a pole in the open air in the sun, even if this is done in the months of July or August the meat outside will remain fresh for weeks, while the meat in the room will spoil in a day or two. This is, of course, in Colorado; but I think the analogy between this meat test and a septic process in the lungs, a very close one. The piazza life in Colorado can be made most attractive and comfortable, and a piazza can be arranged, as one in my own house is, with a stove and blinds, furnished with rugs, bed, etc., and enclosed with glass on the north. In this way we offer not a task, but an inducement to be out of doors; and the nights also can be spent in pure air.

A life on a ranch in Colorado would seem to offer to the invalid an ideal method of being in the air and light, and I have at present some cases who are certainly improving rap-

[1] "Environment in its Relation to the Progress of Bacterial Invasion in Tuberculosis." American Journal of the Medical Sciences, July, 1891. Imdeau. Also: Phthisology, by Evans, p. 243. Text-book of Hygiene, Rohr, p 281. Also Transactions of the Colorado State Medical Society, 1894, p. 321, Dr. Bull: "I have attended the Indians both here among the Sioux, and in Idaho among the Bannocks, and I have known many agency physicians and have talked with them. All agree that the older Indians say that they did not have consumption among them when they lived in tepees and in the open air. But as civilization advanced among them the government has constructed log cabins and small frame houses for them, and they have given up the tepee except for a short time in the hottest weather in summer. Now we have consumption the most prevalent disease. More than half of the deaths are from it."—Gardiner. Letter to the New York Sun, April, 1897. "Phthisis Pulmonalis," by Dr. Percy Kidd," Alhutt's System of Medicine, p. 156. "How to Avoid Tubercle," by Tucker Wise, M D., Medical Record, October 22, 1898.

DECEMBER

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	
												O	O	O	
FAIR.	CLOUDY IN MORNING. PERFECTLY GOOD ALL DAY.	SNOWING. WINDY ALL DAY. COULD SIT OUT SEVERAL HOURS IN A PROTECTED PLACE	FAIR.	FAIR.	FAIR.	FAIR.	FAIR.	FAIR.	FAIR.	FAIR.	FAIR.	FAIR. ALL THREE DAYS VERY COLD. TEMPERATURE NOT ABOVE 17½° IN DAY-TIME, DOWN TO 12° BELOW AT NIGHT. ACCORDING TO ADIRONDACK PRACTICE GOOD FOR SITTING OUT IN A SHELTERED PLACE WITH SUITABLE CLOTHING.	FAIR.	FAIR. CLOUDY, BUT PERFECTLY GOOD FOR SITTING OUT FROM 9-4 OR LONGER.	FAIR.

JANUARY

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
											XX			XX	
17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	
		X									O	XX O		O	
FAIR.	FAIR.	CLOUDY A LITTLE WINDY AND RAW. COULD SIT OUT FOR ½ THE DAY.	FAIR.	FAIR.	VERY WINDY COULD SIT OUT FOR ⅓ THE DAY IN A SHELTERED PLACE	FAIR.	FAIRLY GOOD FOR ½ DAY. SNOWING ALL AFTERNOON	FAIR.	SNOWING. WINDY. COULD SIT OUT ½ DAY IN A SHELTERED PLACE	FAIR.	FAIR. VERY COLD	FAIR. FROM 9-2 WINDY AND COLD ALL AFTERNOON	SNOWING. WINDY ALL DAY.	FAIR. VERY COLD	

FAIR. FAIR. FAIR. FAIR. FAIR. FAIR. FAIR. FAIR. FAIR. FAIR. FAIR. FAIR. FAIR. FAIR. FAIR. TOO WINDY AND DUSTY ALL DAY FOR MOST INVALIDS. SOME COULD SIT OUT FOR A COUPLE OF HOURS—SHELTERED. TOO WINDY FOR EXERCISE, BUT GOOD FOR SITTING OUT IN A PROTECTED PLACE.

FEBRUARY

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
XX O	O	O	O XX	O	O	O				O					
WINDY. VERY COLD. ONLY A LITTLE ABOVE 0° IN DAYTIME. BELOW AT NIGHT	FAIR FROM 9-1. VERY COLD. WINDY AND SNOWY ALL AFTERNOON	VERY COLD AND RAW. COULD SIT FOR ¼ DAY IN A SUNNY SHELTERED PLACE	VERY COLD. GOOD FROM 10-2 WIND ALL AFTERNOON	VERY COLD. WINDY ALL AFTERNOON. SNOWING. GOOD FROM 9-1 FOR SITTING OUT	FAIR. VERY COLD. SIT OUT FOR ½ DAY IN A SHELTERED PLACE	VERY COLD. CLOUDY. SIT OUT ½ DAY	FAIR.	FAIR.	FAIR. OUT FOR ½ DAY. COLD AND CLOUDY IN AFTERNOON	VERY COLD. SIT OUT FOR ¼ DAY	FAIR.	FAIR FROM 9-2. AFTERNOON CLOUDY AND DAMP	FAIR.	FAIR	FAIR. FROM 9-1. AFTERNOON CLOUDY AND SNOWING
17	18	19	20	21	22	23	24	25	26	27	28				
	X				XX O				X	X	X				
FAIR	FAIR. A LITTLE WINDY IN AFTERNOON	FAIR.	FAIR.	SNOWING. DAMP ALL DAY	WINDY. VERY COLD. COULD SIT OUT ½ DAY IN A SHELTERED PLACE	FAIR.	FAIR.	FAIR	FAIR. WINDY	FAIR. WINDY	FAIR. WINDY				

MARCH

1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
X	X								O XX						
FAIR WINDY	FAIR WINDY	SNOWING. WINDY. COULD SIT OUT FOR ¼ DAY	FAIR.	FAIR.	FAIR.	FAIR.	FAIR.	FAIR.	COLD. WINDY OUT FOR ½ DAY	FAIR.	FAIR.	FAIR.	COLD WINDY, GALE OUT FOR ½ DAY IN A SHELTERED PLACE	GOOD. FAIR	FAIR.
17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	
XX			XX	XX	XX	XX	XX								
WINDY. FAIR OUT FOR ½ DAY	FAIR.	FAIR.	WINDY. FAIR. OUT ALL DAY IN A SHELTERED PLACE	WINDY. DITTO	WINDY. DITTO	FAIR. WINDY IN AFTERNOON	FAIR. WINDY. OUT IN A SHELTERED PLACE	RAIN. SNOW. HAIL.	SNOWING. VERY COLD	FAIR. MELTING	FAIR. MELTING	FAIR TILL 2. SNOWING. DAMP ALL AFTERNOON	FAIR TILL 1. SNOWING. DAMP ALL AFTERNOON	SNOWING. DAMP ALL DAY	

APRIL

31	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
						X			X	X		X X	X	X	X
FAIR	FAIR	FAIR	FAIR	SNOWING DAMP ALL DAY	FAIR	FAIR WINDY	FAIR	FAIR	FAIR A LITTLE WINDY ONLY	FAIR A LITTLE WINDY ONLY	FAIR	FAIR TILL 12 M AND AGAIN AFTER 3 P.M. A LITTLE WINDY	FAIR A LITTLE WINDY	FAIR A LITTLE WINDY	FAIR A LITTLE WINDY
17	18	19	20	21	22	23	24	25	26	27	28	29	30		
X	XX														
FAIR A LITTLE WINDY	FAIR VERY WINDY FOR ½ OF THE DAY														

It is only fair to state that the winter of 1898 and 1899 was more severe than for several preceding years. Excess of wind, 4000 miles. Precipitation average, but temperature lower, and snow was kept on ground much longer. From report of P. E. Dondna, Chief of Colorado College Weather Bureau.

According to Adirondack practice only the wind and dust storms are sufficient to keep invalids in the house, provided they can sit on sheltered verandahs such as are used in the Adirondacks, and provided they wear suitable clothing. Not necessary to come in at sunset, I think.

Both fair and cloudy days that are a little windy are marked with a cross. X Very Windy XX Very cold days are marked with a Zero.

O = Cold weather near Zero or between that and 45°, especially after 3 P.M.

Fair days: sunny and pleasant for sitting out

Cloudy

Rain

Wind and dust storms

Snow and wind storms without dust

Some of the days marked fair were varied by occasional clouds. This is especially true of those marked windy with one X or two X's.

idly on ranches; but they do not represent an average invalid, as they are selected cases, especially adapted for the life, with youth, strong digestion, and entire absence of fever, thus making the experiment worth the risk.

I practised medicine at one time for several years among the ranches in Colorado, Wyoming, and parts of Utah, and my experience has been that it is all a well man can do to digest the food that is eaten on most ranches, since it is, with hardly an exception, poor food poorly cooked. Ranch houses, too, are built without regard to sanitation, while the temptations to over-exertion, exposure, etc., are constant. Their isolation is to many a great drawback, meaning, as it does, the difficulty of obtaining medical advice if taken suddenly ill; and no amount of fresh air can compensate for such disadvantages. The life in a boarding-house or in their own home, with the constant use of the piazza, is probably the most practical arrangement for the greatest number of invalids, and is the plan used by at least 90 per cent. of all invalids in Colorado Springs.

I consider that for a certain number of cases a life in a tent is really the nearest approach to the ideal life for a consumptive; I mean, of course, with careful selection of the patient for such a life, and then a careful selection of tent, camping-site, etc., so essential to success. To most people a tent simply means what is ordinarily used in camping out, and it is this limited knowledge of tents that has brought them into disrepute with many physicians. The tent generally used is too low, made of thin duck, often without a floor, without proper artificial heat, and is pitched in the open, becoming like an oven in the Colorado sunshine. Above all, such a tent has no proper system of ventilation, and this is important, since a tent never ventilates itself, as some people seem to think, by air coming through the canvas. The tent I have in mind is the result of considerable evolution in tent building, and although I have not yet been able to demonstrate its use with a large number of patients, I have myself lived in it both in summer and in winter, during rain and snow storms, and for

several weeks at a time. It is made of very heavy duck (twelve ounces), is circular in shape, eighteen feet high, sixteen feet in diameter at the floor, with a wall five feet six inches high. The top terminates in an iron ring one foot in diameter, to which the canvas is fastened, thus forming an outlet for heated air always open. The tent is supported by a centre-pole to which the ring is attached, and the tent can thus be elevated or lowered, or its tension changed from within by pulleys. The floor is raised eight inches from the ground, and is in eight sections, thus being easily moved. The lower edge of the wall is fastened several inches below the floor, one inch from it all around; this is to provide at all times an inflow of air that is gradual and without a draught, since this inch space in my circular tent represents an area of about 600 square inches, and the hole in the top for outflowing air about 123 square inches. In this way the tent cannot be closed, and is constantly ventilated automatically. In other words, it is a circular tent, the bottom of the canvas forming a circle around the wooden floor, but one inch from it all around, and a little below the floor. This open space between the floor and the sides of the bottom of the tent allows air to flow into the tent at all times, while the holes at the top of the tent allows air to flow out all the time. In this way the tent always ventilates itself in any weather, day or night, with the door shut or not, or when heated or not. As the air has to turn a corner to enter the tent, it cannot come as a draught, and as it enters through all the inch space surrounding the tent, it enters slowly and without force, being evenly distributed, but coming through collectively a large area 600 square inches. The tent is easily heated by a wood or coal stove, and yet in warm weather the constant interchange of air keeps it cool even and without a fly. This tent should be fitted up in every way like a room. Cooking need not be done in it, but can easily be provided for at some house near by. Such a tent can be lived in for at least eight months in the year in Colorado at 6000 feet altitude; and in winter can be transported, if neces-

sary, to some warmer climate south. The advantages of a tent life are, plenty of fresh air and diffused light, all of the twenty-four hours, taken without effort. The psychological effects of such a life cannot be told here, but that it is a constant mental rest, that the novelty charms, and that a certain something, possibly the awakening of the hunting and migratory instinct inherited from countless generations of primitive ancestors, do affect beneficially the nervous system of the phthisical, is undoubted.

The points I have endeavored to emphasize are that light and air are to-day the most powerful agents we have for the cure of pulmonary consumption; that this fact is often overlooked, and that attention is paid to cures of much less value; that the most perfect qualities of light and air are only found on high, dry plateaux of over 4000 altitiude, and that, under such climatic conditions, not only is the quality of light and air more perfect than elsewhere, but the available quantity taken is in excess, as it is practical for a patient to be out and obtain it; that if possible a pulmonary invalid should be out doors every hour of the twenty-four, as indoor air is not a cure for phthisis, and that a piazza life can be arranged for and made as comfortable as a room. Ranching, as generally utilized, is a dangerous life for the average invalid, owing to bad food, exposure, over-exertion, etc. A selected number of cases can be induced with benefit to live a tent life, provided the tent is properly selected and managed on the plan I have suggested.

Thus, while avoiding the ills of civilization from which our patient is escaping, we so control the new environment of nature as to avoid all its dangers and obtain all its benefits.

DISCUSSION.

Dr. W. F. R. Phillips, of Washington, D. C.: I am gratified to hear Dr. Gardiner's paper, especially as he emphasizes a point which has hitherto been very much neglected; namely, the effect of light in the climatic treatment of consumption. I have been endeavoring for several years to study the physiological effects of light upon the human organism, and it has impressed me as being the most important factor in the climatic treatment in high altitudes. So far as the rarefication of the air in such regions is concerned, that, it has always seemed to me, would be injurious rather than beneficial. It compels a man to breathe more rapidly and to expand his lungs more in order to get the necessary amount of air, and it has never appeared rational to me that we should require lungs which are diseased to do an extra amount of work. Altitude, I believe, has been credited with what is justly due to light, and for that reason I am particularly pleased that Dr. Gardiner has brought out this point so strongly. I believe the subject is one that requires further investigation. We have already observations to show that light increases the hæmoglobin of the blood, and hence combats the anæmia, and this beneficial change has hitherto been attributed largely, if not exclusively, to the effects of altitude. In elevated plateaux, where cloudy weather is rarer than in lower regions, the action of light is greatly intensified, and naturally its beneficial effects are more pronounced.

Dr. E. O. Otis of Boston: I would like to ask Dr. Gardiner the average number of hours, week in and week out, that he is able to keep his patients out on the piazza? Also, how great a control does he exercise over his patients?

Dr. Gardiner: I find it difficult to exercise proper control over my patients, otherwise I should be able to keep them out in the air much longer than is now the case. At present my patients average five and one-half hours daily in winter, probably much more in summer.

Dr. W. D. Robinson, of Philadelphia: The benefit derived by these patients in high altitudes is probably due to a combination of the light and elevation. In such regions the peripheral blood circulation is increased, thereby exposing an increased volume of blood to the effect of light.

Dr. N. S. Davis, Jr., of Chicago: I wish to express my personal conviction that climate has very much to do with the promptness and efficiency of the outdoor treatment of tuberculous patients. During the last few years the pendulum of opinion has been swinging toward the view that tuberculous patients can be treated in almost any climate, providing they can be kept out of doors more or less continu-

ously. It is true that efficient treatment may prove successful in any climate. For example, in the early days of Chicago, when it was still a frontier town, patients were sent there from the East, and many cures of consumption were effected. It is true that tuberculosis may be practically cured in any climate, but it is also true that there are certain climates where a cure is much more likely to be effected than elsewhere. Colorado and similar localities undoubtedly afford the best climate for the treatment of tubercuIosis.

Dr. Samuel A. Fisk, of Denver: While I fully agree with what Dr. Gardiner has told us regarding the beneficial effects of light and air in Colorado, of which I see almost daily evidence, I would not under-estimate the value of nutrition in the treatment of these cases. Air without food, and proper food, is insufficient. One is as important as the other; they are the twin sisters that go hand-in-hand, and the best results, in my experience, are to be obtained when we regard the nutrition of the patient as well as the air he breathes. Only a few days ago a patient from the East came to me, and I asked him what directions he had received regarding his diet. He said, "Dr. So and So said nothing to me about my diet, excepting that I should eat six raw eggs daily." In other words, his physician was looking after the nourishment of his pateint.

Much can be said on the subject of air. When you once get a Colorado man talking about that, he is apt to talk you to death, so I will simply close my remarks by emphasizing the fact that the nutrition of the patient should also be borne in mind.

Dr. S. E. Solly, of Colorado Springs: In connection with the general subject under discussion, I would like to say that when a fact is presented or a statement made in connection therewith, it should be supported, so far as is possible, by evidence. What we want in this Association is evidence to strengthen our statements. The less we have to do with glittering generalities, such as those presented by the first gentleman who discussed Dr. Gardiner's paper, the better. He said so and so, and so and so, instead of giving us the result of his clinical experience, or the facts on the subject contained in the literature. We do not want to tie ourselves to light alone, or altitude alone, or to anything else in the treatment of tuberculosis. We want evidence; we want these gentlemen to work and dig into the literature of the subject. Let us investigate the work of others or do original work ourselves, and I wish to enter a protest against any general statements of this kind.

Dr. Phillips: In reply to Dr. Solly, who I believe referred to me, I would say that I believe I could demonstrate to him the truth of the statements I made regarding the effects of light.

Dr. Solly: Oh, you are Dr. Phillips; I did not recognize you when you first spoke.

SUGGESTIONS CONCERNING EARLY DIAGNOSIS IN PULMONARY TUBERCULOSIS.

By S. G. BONNEY, M.D.,
DENVER, COLO.

Few subjects of recent years have attracted greater interest in the profession than the general problem of pulmonary tuberculosis. With a view to the ultimate lessening of the disease, medical attention has been directed to the advocacy of its prevention, restriction, and control through governmental authority and provision. More exact knowledge concerning its contagiousness has been promulgated among the laity by the dissemination of educational literature. Concerning certain special methods of treatment, much has been written by enthusiastic and occasionally conscientious observers. Greater recognition is being given to the general conservative measures of management, the adaptability of various climates to the different stages of the disease, and a greater appreciation accorded the beneficent influence in the community of carefully conducted sanitoria.

Yet the unfortunate fact remains that thousands of lives are being sacrificed annually on account of mistaken or delayed diagnosis. It is now generally accepted that consumption is a curable disease, in the sense of its permanent arrest; that the earlier the diagnosis the better the prognosis; and that only in early cases are genuinely satisfactory results obtained. The time is coming when the profession will not accept the somewhat vague reports of observers concerning various degrees of improvement secured from climatic change, serum-therapy, or general management. Rather

will it demand more definite statements as to permanent results, with special reference to total arrest, shown by the well-substantiated absence of rational symptoms and physical signs. Such will be the basis in the increasing multiplicity of reports upon which authentic conclusions will eventually be reached. Recorded results of such a satisfactory nature will never be rendered save in those cases where the diagnosis is made early and the cases come under trained observation in their incipiency. Generally speaking, it has not been such a class of cases that I have been privileged to observe in Colorado, where the advanced case is the rule and not the exception.

Early diagnosis, then, assumes a position in the general consideration of pulmonary tuberculosis of vastly more practical importance than climatic change, general management, or prophylaxis. It is true that perfect accuracy of diagnosis may be established by the recognition of the tubercle bacillus, but not always before the destructive process has already become considerably advanced, the constitutional disturbance pronounced, or even the development of secondary infection. I have not infrequently failed for a considerable period to discover the bacilli in the sputum in cases where the physical signs furnished indubitable evidence of a recent and not inactive tuberculosis. In other cases the significance of the symptoms and signs has been incontrovertible before the appearance of expectoration. Due recognition is given to the diagnostic value of the tuberculin reaction. Recent investigations with the Röntgen rays disclose their utility for diagnostic purposes in thoracic disease. Neither of these, however, by reason of the necessary restrictions upon their employment, are ordinarily within the immediate reach of the general practitioner. The claim has been made for the diagnosis of the so-called pre-tubercular state from the morphology of the blood, but has thus far lacked satisfactory confirmation. In view of the frequent obvious difficulties for the clinician in establishing an exact and early diagnosis through the agency of the microscope, the Röntgen rays, and the tuberculin of

Koch, is it not profitable at this time to review the well-known and perhaps old-fashioned principles of diagnosis with the aid of the stethoscope and thermometer, the careful and painstaking application of which are too frequently overlooked? The distressing results of error and delay have not been occasioned by inability to apply the newer diagnostic principles, but on account of the failure to adequately appreciate the significance of rational symptoms, to recognize accurately the physical signs and to properly interpret their import. In the majority of instances the available data for diagnosis has been amply sufficient to warrant its provisional establishment and demand suitable advice as to mode of life and residence long before the medical attendant has awakened to a realization of his responsibility.

As illustrative of this I will refer briefly to observations made from own experience. In reviewing my recorded cases of pulmonary tuberculosis I have selected therefrom a series of 546 seen in private practice and continued under observation for a considerable period in Colorado, although contracted elsewhere. Three hundred and eighty-eight, or 71 per cent., arrived in this State with distinct evidences of tubercular infection in each lung.

With reference to the duration of the disease before arrival, my statistics, computed from a conscientious inquiry concerning the time of onset of definite symptoms characteristic of the disease, are as follows:

Less than 3 months thereafter	36
3 to 6 " "	65
6 to 9 " "	67
9 to 12 " "	70
12 to 18 " "	83
Over 18 " "	225
	546

The total average period of delay in 546 cases is a little over eighteen months. affording abundant opportunity for advanced pulmonary and constitutional impairment. Since

the compilation of these statistics I have found that Dr. Solly, in an admirable paper several years ago, containing an analysis of one hundred cases, states that "the total average of delay was two years."

The average loss of weight from the normal at time of arrival is nineteen pounds.

Sixty-four and one-half per cent. presented marked constitutional disturbance, as evidenced by a considerable rise of temperature, feeble and rapid pulse, distressing cough with abundant expectoration, impaired appetite, and a correspondingly greater loss of weight. Nearly 62 per cent. of this class did not seek climatic change until after one year had elapsed following the development of the disease, and 80 per cent. until after six months.

One hundred and five, or 19.2 per cent., had well-defined cavities, nearly all of these exhibiting pronounced rational symptoms of constitutional disturbance, with rather more than the average loss of weight and an average duration before arrival of twenty and one-half months.

Two hundred and seventy, or 47.6 per cent., presented a family history of strong inherited susceptibility to tuberculosis.

Concerning the method of onset, eighty-three, or 15.2 per cent., upon careful investigation may be safely considered to trace the origin of the disease to a distinct attack of influenza. The average duration of this class before arrival was a little over twelve months—much shorter than the period for the total number—while the average loss of weight was twenty and nine-tenths pounds. It is perhaps interesting to note, somewhat irrelevantly, that the number of such cases has materially diminished during the past few years, the proportion in the first 200 cases—from 1892 to 1895, inclusive—being 19 per cent.; during 1896 and 1897, 18 per cent.; and for the 200 cases in 1898 being but little over 9 per cent.

One hundred and twenty-nine, or over 21 per cent., stated the onset to be of a definite bronchitic character, with an average duration of fifteen months and twenty-four days,

following the inception of a severe cold. The average loss of weight was seventeen and one-tenth pounds.

One hundred and fifty-nine, or 29 per cent., are described to be of gradual, anæmic onset, with an average period before arrival of nearly two years. The loss of weight was seventeen pounds.

One hundred and fourteen, or 20.2 per cent., presented the history of a sudden hemorrhage as the first symptom distinctly referable to the disease. Of this class the average loss of weight was seventeen and six-tenths pounds and the average duration before arrival was two years three and one-half months—much longer than the average of the total number.

Exclusive of those with definitely hemorrhagic onset, 134 had suffered from one or more hemorrhages in the subsequent course of the disease before arrival.

Sixty-one are unclassified with reference to the method of development, a very considerable number of these following measles, pneumonia, typhoid, and pleurisy.

Singularly, an equal number, sixty-one, are found to have experienced a distinct idiopathic pleurisy, either preceding the apparent origin of the pulmonary process, occurring as an initial manifestation, or developing subsequently. In fourteen of these an effusion had been recognized. The average period following the pleurisy before arrival was thirty-sixth months.

Is not the character of the cases sent to Colorado a striking commentary upon the necessity of a more thorough appreciation of the principles capable of everyday application upon which to base early diagnosis? One of the significant features of the analysis is the relatively shorter period before arrival, especially in those cases beginning with influenza, and to a less extent in those with bronchitic origin, as compared with the longer duration of those with anæmic and more particularly hemorrhagic onset. It would seem to be apparent from this :

1. That cases beginning with an acute attack of influenza

pursue a more rapid course, according in general earlier diagnosis and demanding more prompt climatic change.

2. That the occurrence of a hemorrhage does not convey to the mind of the medical attendant, as would naturally be supposed, its full and almost startling import as a diagnostic feature.

It is not my purpose to attempt a wearisome recapitulation of the rational symptoms and physical signs of pulmonary tuberculosis, so familiar to us all. I am impressed, however, from a constant and more or less intimate association with pulmonary invalids, that there are a few factors of recognized importance which by the busy practitioner are frequently ignored.

As regards the rational signs, the non-appreciation of the significance of morning cough, even without expectoration, persistent daily rise of temperature, loss of weight, and other well-known evidences of constitutional impairment would indeed appear inexcusable. But how often do we find denial or absence of cough, periods of normal temperature, and even recent increase of weight, in the presence of fairly definite physical signs, in the same way that we may fail to detect satisfactory evidence of the disease despite rather pronounced subjective symptoms? It is apparent that the presence or absence of no single rational sign can be safely accepted as a factor of paramount diagnostic value. The grouping of certain clincial manifestations, without other satisfactory cause, should lead to strong suspicions of a concealed focus of tuberculosis despite immediate absence of physical signs. It must be added, however, that in the majority of instances the sings are present and recognizable, once a thorough examination of the chest is made.

As preliminary to a detailed consideration of the rational signs on the part of the medical adviser there should be instituted a careful inquiry as to the extent of inherited susceptibilitiy, the history of the individual with reference to the previous existence of idiopathic pleurisy, with or without effusion, an apparently trivial or remote pulmonary hemor-

rhage, the occurrence of la grippe, a succession of colds, a protracted convalescence from typhoid or malaria, the history of a slowly resolving pneumonia, a recent severe whooping-cough or measles, and other factors of etiologic importance. The development of indefinite symptoms following a history of any of these conditions should be regarded with grave suspicion. The initial occurrence of hemorrhage without readily assignable cause should furnish, even in the absence of all clinical manifestations or physical signs, a strong suggestive indication of the existence of an already active though limited focus of tubercular infection. Furthermore, a pulmonary hemorrhage supervening in the course of other rational symptoms, without physical evidences, should afford convincing testimony concerning the actual nature of the existing pulmonary process. The occasional dry but persistent, unexplained cough, even unassociated with other rational symptoms, should offer at least a clear indication for continued medical observation and repeated examinations. While the absence of an elevation of temperature constitutes no argument for the exclusion of the diagnosis of a tubercular deposit, its presence under certain conditions is assuredly an element of exceeding value. The characteristics of temperature of more especial significance, and which can only be appreciated by repeated use of the thermometer at two or three-hour intervals, in suspected cases, during a period of several weeks, are its irregularity and atypical course, an average for the day slightly above normal, its usual though not invariable rise in the evening, and absence of corresponding systemic disturbance.

The thorough and painstaking examination of the chest immediately upon the development of suspicious symptoms, and its frequent repetition, if necessary, will usually suffice, through the detailed application of the principles of auscultation and percussion, in establishing for clinical purposes a sufficiently early diagnosis. As a matter of fact, such an examination is seldom made until a provisional diagnosis has become apparent from the rational symptoms, and the

consultant at once recognizes physical evidences of advanced infection. Is it not, then, of practical importance, prefatory to a consideration of the more recent aids to the recognition of consumption, to continue to emphasize a more strict and conscientious adherence to the principles of physical diagnosis now admittedly ignored in so many instances? Apropos of this I would especially condemn :

1. Delay in instituting any physical examination whatever until long after the development of pronounced constitutional and pulmonary impairment.

2. Failure to examine on the bare skin, the presence of clothing effectually preventing any approach to accurate results.

3. Neglect to examine the entire chest, the bases, interscapular spaces, and axillary regions being frequently overlooked.

I would depore particularly the existence of :

1. Erroneous conceptions concerning the significance of the absence of percussion dulness at the apices, an active process often being capable of recognition by auscultation considerably before the evidences of consolidation are apparent.

2. The non-recognition, in the absence of râles, of the various modification in disease of the normal respiratory sounds.

3. Inaccurate interpretation of localized diminished intensity of auscultatory sounds, elevation of pitch, harshness of quality, and prolongation of expiration.

I would severely criticise :

1. Failure to utilize cough preceding forced inspiration in eliciting the presence of slight moisture in the finer tubes.

2. The non-appreciation of the almost pathognomonic significance of a circumscribed bronchiolitis, even in the absence of dulness or other auscultatory signs.

DISCUSSION.

Dr. Frederick I. Knight, of Boston: I wish to repeat here the statement which I made in my remarks on this subject last year; namely, that the early diagnosis of pulmonary tuberculosis should not be made for the sake of sending your patient to Colorado or anywhere else, but that it should be made in order that the proper treatment can be instituted at once.

In regard to the presence of bacilli in the sputum in the earlier stages of the disease, I must say that my experience rather differs from that of Dr. Bonney. I have very rarely failed to find the bacilli in the sputum of patients who present themselves to me, and I see many cases early in the course of the disease. It is true that they are patients with cough, and usually with a little expectoration. The tubercular disease that develops without cough naturally falls into the hands of the general practitioner.

According to Dr. Bonney's statistics, the hemorrhagic cases are the ones which delay longest before going to Colorado. This is interesting and is in line with the pretty generally admitted fact that those cases pursue a longer and usually a more favorable course than the other types of the disease. They for a long time may complain of very little cough or expectoration, and the disturbance of their general condition is not severe enough, in their opinion, to oblige them to consult a physician or adopt any radical measures of treatment.

Dr. R. H. Babcock, of Chicago: The doctor's paper was so complete that it would seem as if there was hardly room for anything more than favorable comment upon it. Nevertheless, I should like to speak of what struck me particluarly, and that is the very large percentage of his cases, namely, 71 per cent., who had been kept at home for varying periods before they were sent away for climatic treatment. The doctor's conclusions were that these cases remained at home chiefly or largly because of delayed diagnosis. That, unquestionably, is a correct conclusion in many of them, but is it a legitimate one in regard to all of them? Is it not true that a very large number of patients are kept at home in the care of the family physician for some ulterior motive than the patient's good? Is it not true that many are kept at home because the family practitioner does not wish to lose sight of the case? This, perhaps, is a very pessimistic view of the matter, and attributes to the rank and file of the profession methods which are unworthy, and perhaps I should apologize to the profession for even insinuating so much, but I believe it is impossible for the rank and file of the profession, no matter how ignorant

they are about physical diagnosis, to keep these patients under their care for eighteen months without recognizing their true condition.

Another motive, which is not an unworthy one, is the belief on the part of the attending physician that his patient will improve at home under proper treatment, and that it is unnecessary to send him away. If that is the reason for the delay, then the 388 cases mentioned by Dr. Bonney furnish a striking commentary on the incorrectness of such an opinion. For my part I insist upon the importance of an early diagnosis for the very purpose of sending the patient away to Colorado. I believe so sincerely that a change of climate will restore such patients in the incipiency of the disease that, as soon as I recognize a case in its beginning, I promptly advise such patients to go away. If that advice cannot be followed, then some other advice is given.

In the early diagnosis of pulmonary tuberculosis I should like to dwell on the importance of temperature. A study of the temperature should never be neglected. We all see cases where the early diagnosis of the disease is difficult, on account of the vagueness of the physical signs. There may not be a rise of temperature in all cases, but there is so apt to be a slight rise, perhaps only of one degree, that if the temperature is studied carefully for a week or more, the physician can usually come to a correct conclusion.

I am not a laryngologist, but I must say that I am skeptical about the statement just made, that a careful laryngoscopic examination will anticipate a diagnosis of pulmonary disease before it can be detected in the lungs. I have friends at home who I know are skilful laryngologists, and I have so often called on them to help me out that I cannot believe their failure to recognize a change in the larynx was due to ignorance, but rather that such changes were not there. I have sent patients with incipient disease of one or both apices to laryngologists, and the statement came back that the larynx was practically healthy.

In regard to the significance of hæmoptyses, I should like to say that I believe they are nearly always the precursors of pulmonary disease or due to it.

Dr. Richard C. Newton, of Montclair: In confirmation of what Dr. Babcock has just said about laryngeal phthisis, I believe the fact is generally admitted—for which we should thank Heaven—that it is a comparatively rare, although a distressing, intractable, and, in fact, a well-nigh hopeless phase of consumption.

Dr. Roe: The examiner must make the distinction between a tuberculous laryngitis and the laryngitis of tuberculosis.

Dr. C. F. McGahan, of Aiken, S. C.: I agree with Dr. Roe, that an examination of the larynx is often of great value in the early diagnosis of these cases. It is my custom to examine the larynx in

all my cases, and I have now a patient under observation where the larynx is suggestive of tuberculosis, although I can find nothing in the lungs.

Dr. E. O. Otis, of Boston: Dr. Bonney, in his paper, emphasized the importance of the inherited susceptibility of the patient in the acquirement of pulmonary tuberculosis. In connection with that I think the question of exposure to the disease should be carefully investigated in each case. Last year Dr. Loomis, in a valuable paper read before this Association, spoke of two other factors in making an early diagnosis, namely, the vital capacity of the individual as determined by the spirometer and the lowered arterial tension. These I believe will often give us valuable indications in making an early diagnosis.

Dr. Phillips: I would like to inquire whether any of the members have noticed a subnormal temperature in tuberculous patients? In the incipient stage of the disease, it seems to me, that is almost of as common occurrence as a slight rise in temperature.

Dr. Knight: There is one point in regard to the larynx which should be brought out. I think all laryngologists agree that in incipient pulmonary tuberculosis the larynx may have a suspicious appearance—not an ulcerated condition, but a certain indefinite appearance which has hitherto not been fully described, and which Dr. Roe can, perhaps, describe to us more in detail.

Dr. Roe: It is rather difficult to describe the condition off-hand. You often see it intuitively—you know that what you see is indicative of a certain condition without always being able to explain why. In the laryngitis of tuberculosis—not tuberculous laryngitis, which almost anyone who can use the laryngoscope can recognize by the peculiar pyriform swelling which we do not see in the former—in the laryngitis of tuberculosis we usually have an atrophy of the arytenoids and sometimes of the entire mucous membrane of the larynx, which is very marked. There is also pallor, especially about the epiglottis, and the general appearance of the parts will enable one to recognize that there is a change in the circulation of the lung.

Dr. S. W. Langmaid, of Boston: That there is such a condition of the larynx as Dr. Roe has described, I am sure, but that it is surely indicative of tuberculosis of the lungs, I am doubtful. It is a symptom of anæmia, and if we look upon it as a corroborative symptom, I think we shall not be led astray. A change in the voice and a changed appearance of the glottis should also be so regarded, as they indicate that the nervous force is already much diminished.

Dr. Roe: I agree with Dr. Langmaid, that the appearance of the larynx in these cases can only be regarded as an additional or corroborative symptom.

Dr. Guy Hinsdale, of Philadelphia: In considering this subject

I think we should not forget the beautiful demonstration of early tuberculous lesions in the lungs given by Dr. Williams through the fluorescent screen at our last meeting, and supported by the paper of Dr. Stubbert on the same subject. I think they were most striking examples of the early diagnosis of pulmonary tuberculosis.

Dr. Babcock: I was only expressing some skepticism concerning the doctor's statement that the appearance of the larynx sometimes indicated pulmonary tuberculosis before an examination of the lungs would show it. I do not question for a moment that the appearance of the larynx is significant when pulmonary changes are present, but I was surprised that the larynx should indicate pulmonary tuberculosis at so early a stage that careful examination of the lungs would not reveal the presence of the disease.

Dr. Roe: I did not say it was indicative of pulmonary tuberculosis, or, at least, what I meant to say was that it was indicative of approaching trouble in the lungs.

SOME NOTES UPON THE TUBERCULIN TEST.

By EDWARD O. OTIS, M.D.,
BOSTON.

So long as the tuberculin test has still many opponents as well as ardent adherents, all added evidence is of value in arriving at a final estimate of its worth in the diagnosis of early or doubtful tuberculosis. I offer my experience all the more readily from the fact that no contribution upon the subject has, to my knowledge, been presented to this Association. The problem to be solved is a twofold one: First, is the test injurious as now used? and, second, is it reliable?

Those who have used it and still continue to use it are quite unanimous in answering these questions in the affirmative, and the experience of the writer accords therewith. Such are Krause, Gaffie, Von Jaksh, Grasset and Wedel, Springthorpe, Cornet, Landouzy, Max Beck, and many others abroad; and Trudeau, Whittaker, F. W. White, A. C. Klebs, R. C. Cabot, and others in this country.

I cannot help suspecting that those who consider the test dangerous even in the small dose now used may be prejudiced by the unfortunate results which occurred in the early history of tuberculin from the enormous doses then used. We all look back upon that epoch with a shudder. To pass it by with the remark "that its value as a test is not great, as reactions are obtained in several other diseases," as Fowler[1] does, it likewise seems to me unfair and misleading. Further evidence, to be sure, is needed to establish its exact position as a diagnostic method, but enough has already been adduced to prove its success in the majority of cases. Its rival is the X-ray, but that requires an expert and an expen-

[1] "Diseases of the Lungs," Fowler and Godlee, 1898, p. 398.

sive apparatus, which limits its use to the few. Any one can use the tuberculin test.

In view of the extremely favorable curative outlook the early case of pulmonary tuberculosis offers from the present methods of treatment, the profession has never been so eager to detect the beginning of the disease as at present, and any aid in accomplishing this should be welcome, limited though it may be. We all recognize the fact that auscultation and percussion and the sputum examination fail to do this in some, we know not how many cases. If we delay until tubercle bacilli are detected in the sputum the favorable opportunities of the first stage may have passed, never to return.

Before an absolute diagnosis has been established one is naturally disinclined and lacks the requisite authority to institute a vigorous plan of treatment. Certainty of facts renders one resolute and swift in action. Here, then, is a test which is so simple that any one can apply it, and yet in the majority of cases settles the whole question of diagnosis definitely and at once. If we shall in the future be able to determine some more exact method of dosage, discovering some constant ratio between the dose and the individual, we may find the test to be true in every case.

In common with others, I have injected cases of proved pulmonary tuberculosis which did not react, at least in the general reaction, and, on the contrary, it has happened to me to obtain a more or less complete general reaction when I could not feel convinced that any tuberculosis existed. It may be that above a certain dose, maximum to the individual in question, what at least simulates a general reaction occurs in a healthy person, a temporary poisoning by the tuberculin and its toxins; further evidence, however, is necessary to decide this question.

My observations extended over 111 cases, originally undertaken in an ambulatory clinic for the purpose of arriving at a conclusion as to the proportion of cases of cervical adenitis that were tuberculous. While making these investigations I embraced the opportunity to test all the cases which for any

reason suggested tuberculosis, as well as several cases of syphilis and a variety of other cases. My results of last year were published in the *Medical News* of July, 1898, and the tables of them, as well as those of this year, I append to this communication. In the total number of fifty-six cases of cervical adenitis taken without selection there were thirty-three reactions, six slight reactions, and two doubtful ones. Throwing out the slight and doubtful ones, we have 58.8 per cent.; including them, 73.2 per cent., or an average of 66 per cent., which would indicate the proportion of cases of cervical adenitis that were tuberculous, so far as an inference can be drawn from fifty-five cases and dependence can be placed upon the tuberculin test; but of course a larger number of cases and other methods of investigations must corroborate or disprove these deductions.

It seems not unlikely, however, that this is not far from the truth, for Volland makes the proportion 68 per cent., and Dr. F. C. Moore,[1] out of twenty-eight cases of chronic enlargement of the glands, mostly of the neck, which had to be operated upon for various reasons, found that 73 per cent. were tuberculous. In eight cases of syphilis and one doubtful one there were four reactions. One of these cases was injected for cervical glands, and reacted moderately after 5 milligrammes and markedly after 10 milligrammes. A few days later evidence of secondary syphilis appeared. Another, of chronic enlargement of the metacarpal bones of two fingers of the left hand, which was diagnosticated by one surgeon as syphilis and by others as tuberculosis, reacted after 5 milligrammes, still leaving the diagnosis in doubt. There does not appear to be any doubt, then, that a certain proportion of syphilitic cases will react. This fact, however, would rarely interfere with the test in its more useful application, viz., in suspected early tuberculosis. In seven cases of more or less advanced pulmonary tuberculosis containing tubercle bacilli in the sputum, three gave no general reaction after 10 or 12 milli-

[1] London Lancet, September 17, 1898, p. 734.

grammes, and one none after 5 milligrammes. Unfortunately, the local conditions after the test were not noted.

Of course, the only deduction that can be drawn from these few cases is that pulmonary tuberculosis, when more or less advanced, will not always give a general reaction from 5, 10, or 12 milligrammes of tuberculin; it is well to remember, however, the fact, which White calls attention to, that the general reaction is slight and the local reaction marked in advanced cases, while in the early cases, where the test is most useful, the reverse is true. In the other general cases the results corroborated the clinical diagnosis in the majority of instances. Whether any reaction occurred without the existence of tuberculosis one can only conjecture, but it is rather surprising that atrophic rhinitis and pharyngitis sicca should give a reaction without other evidence of tuberculosis. In no case did any serious result follow, although in several the general reaction was severe and accompanied with much temporary depression.

The clinic being an ambulatory one, the subjective evidence of the patient, together with the objective symptoms apparent when he presented himself the next day at the clinic, were mainly relied upon to determine whether or not a general reaction had occurred. If from six to twenty-four hours after the injection the patient complained of excessive weakness, sensations of heat and cold, nausea, anorexia, pain in the back and limbs, severe headache, sweating, either sleeplessness or somnolence, epitomized by him as feeling "very sick," "awful bad," or "miserable," and he appeared the next day with coated tongue, rather a rapid, weak pulse, and a general appearance of marked depression, a general reaction was considered to have occurred, even though the temperature at that time was not much, if any, raised. The cases were generally afebrile at the time of the test.

Objection may be urged as to the accuracy of these tests when the patient could not be constantly under observation, as in a hospital ward; but any one who has listened to the graphic recital of the reaction cycle and witnessed the evidence

of weakness and depression exhibited by the patient would, I am sure, be convinced that a general reaction had occurred. Of course, all reactions were not of the same intensity or duration.

Moreover, since making my first set of tests, a year ago, Dr. R. C. Cabot,[1] in the out-patient department of the Massachusetts General Hospital, has followed a similar plan in tuberculin and other injection tests, and has satisfied himself also that accuracy of results can be attained in an ambulatory clinic. As a matter of convenience, I gave the injection in the arm, generally subcutaneously, and the site of injection was swollen and painful for a few days. This annoying sequela can generally be avoided by making the injection deep in the muscles. Either Koch's original tuberculin or that made in the Adirondack Cottage Sanitarium Laboratory, kindly furnished me by Dr. Trudeau, was used. As the latter seemed to be less concentrated than the former, judging from results, I confined myself finally to Koch's, previous experience with which had taught me what to expect, from certain doses.

One is likely to be misled, I think, if he uses indiscriminately tuberculin of different concentrations, and therefore he can better estimate his results if he has a single preparation of uniform strength. I diluted the Koch tuberculin to a 1 per cent. solution, using either distilled water or a normal salt solution. Unless one makes a fresh solution every day or two, the addition of a few drops of carbolic acid is desirable. With a pipette graduated into tenths and hundredths of a c.c. milligrammes can be easily measured.

As to the dose, there is much diversity of opinion. Some assert that they obtain satisfactory results with very small doses, as Grasset, with two to five-tenths of a milligramme for an adult, and Gaffie, one-twentieth of a milligramme for infants. The majority, however, use larger doses, from one-half to ten milligrammes, and occasionally twenty. A. C. Klebs regards twenty milligrammes as the maximum dose

[1] "Substitutes for Tuberculin in Diagnosis," by R. C. Cabot, M.D., Journal of the Boston Society of Medical Sciences, January, 1899.

which can be injected safely, but he would not begin the test with this dose. It is not unlikely that this difference of opinion regarding the dose is largely due to the difference in the strength of the preparations used. I have never used over 12 milligrammes of Koch's tuberculin, generally from 5 to 10 milligrammes for an adult and from $\frac{1}{2}$ to 3 milligrammes for children. At the Massachusetts General Hospital in Boston 7 milligrammes is their usual dose, I believe.

I would summarize my conclusions as follows, subject to modification by further experience:

1. The tuberculin test indicates early tuberculosis by a general reaction in the majority of cases before it can be detected by other methods, the X-ray excepted.

2. The dose to accomplish this is from 5 to 10 milligrammes of Koch's original tuberculin.

3. No injurious results occur from the use of tuberculin in these doses.

4. Proved tuberculosis in a more or less advanced stage may fail to give a general reaction with doses of 10 or 12 milligrammes.

5. Syphilis gives a reaction in an undetermined proportion of cases.

6. A non-tuberculous person may give a general reaction with a dose above the maximum used in the test.

7. The reaction may be delayed from six to twenty-four hours.

And as rules to be observed in making the test:

1. Always use the same tuberculin and of a standard strength.

2. Use aseptic precautions in giving the injection.

3. Make the injections deep into the muscles.

4. Keep a two, three, or four hourly chart of the temperature if possible, beginning twenty-four hours before the injection.

5. Allow several days to elapse before repeating the test.

6. In early cases depend upon the general reaction; in later cases, if the general reaction is wanting, carefully look for the local.

Table I.—Cervical Adenitis. 1898.

No.	Condition.	Duration.	Sex.	Age.	Dose in mg.	Result.
1	Enlarged glands under right ear	2 months	M.	18 years	1, 3, 10	No reaction.
2	.....	.....	M.	6 "	1, 2	" "
3	Suppurating gland in left neck	1 month[1]	F.	10 "	1, 2	Reaction after 2 mg.
4	Cervical abscess in left neck	.....	M.	3 mos.	1/10, 1/5	No reaction.
5	Indurated mass under sternomastoid running up into region of the parotid. Has decayed teeth	1 week	F.	40 years	2, 5, 10	Reaction after 10 mg. Swelling of joints of hands and feet persisting.
6	Chronic suppurating gland in left neck	2 years	F.	23 "	5	Reaction.
7	Enlarged submaxillary gland on right neck	3–4 years	F.	18 "	4	"
8	Glands on right neck, one suppurating five months	2 years	F.	11 "	2	"
9	Small glands both sides of neck, in occipital region	3 weeks	F.	.....	3, 5	No reaction.
10	Mass of 6 to 7 glands extending from the left mastoid downward along the muscles	10 months	F.	20 "	5	Reaction.
11	Two glands in left neck below jaw	3 weeks	F.	23 "	5, 10	Reaction after 10 mg.
12	Two glands in left neck	2 "	F.	9 "	3	No reaction.
13	Suppurating glands in left neck; has had two others within last two years	.....	F.	31 "	5	" "
14	Two discrete glands under sternomastoid; old scars from others	.....	M.	7 "	1, 3	Reaction after 3 mg.
15	One discrete submaxillary gland	2 weeks	F.	23 "	5	Reaction.
16	Cervical submaxillary glands	1 year	F.	9 "	4	"
17	Glands in left neck; has had others on both sides	.....	M.	24 "	3	"
18	Glands in occipital region	6 weeks	F.	19 "	3	"
19	Two glands in left neck	2 "1	M.	14 mos.	?	Doubtful reaction.
20	One gland in left neck	1 week	F.	12 years	1/2, 1, 2	" "
21	Scar from old gland in left neck	.....	F.	7 "	1, 2	No reaction.
22	Glands in left neck; has had others	8 months	F.	16 "	3	Reaction.
23	One large gland below angle of left jaw; has had others	6 years	F.	14 "	3	
24	Three large fluctuating glands in left neck; has had others	6 months	M.	27 "	5	
25	Glands in right neck	8–9 years	F.	21 "	5	
26	Glands in right neck	.....	F.	18 "	3	
27	Enlarged submaxillary gland, left side	2 weeks	F.	19 "	5	No reaction.
28	Glands in left neck and in left axilla	2 years	F.	20 "	3	Reaction.
29	Enlarged submaxillary gland, left side	1 week.	F.	35 "	3	No reaction.

[1] From the Medical News, July 9, 1898. "The Tuberculin Test in Cervical Adenitis." Edward O. Otis.

Table II.—Cervical Adenitis. 1899.

No.	Condition.	Duration.	Sex.	Age.	Dose in mg.	Result.
1	Enlarged glands on both sides of the neck	3 weeks	F.	3 years	½	Slight reaction.
2	Suppurating gland in right neck; possible previous syphilis	2½ mos	M.	24 "	10	Reaction retarded twenty-four hours.
3	Suppurating gland in left neck	3 years	F.	6 "	2	No reaction.
4	Enlarged gland in left neck	2 months	M.	5 "	1	" "
5	Small gland at the left inferior maxillary angle	6 weeks	F.	8 "	2	Reaction moderate, delayed twelve hours.
6	Multiple glands in left neck, also a few small ones on right side	Uncertain	M.	9 "	5 mg. T.[1]	Slight reaction.
7	Enlarged glands in right neck, one fluctuating; has had them before	.....	F.	12 "	10 mg. T.	" "
8	Enlarged submaxillary gland in left neck	10 months	F.	43 "	8 mg. T.	" "
9	Enlarged glands, right side	1 week	F.	15 "	5 mg. T.	Reaction.
10	" " " "	3 weeks	M.	43 "	10 mg. K.[2]	"
11	" " " "	3 "	F.	19 "	5, 10 mg. K.	No reaction.
12	Glands on both sides of neck	1 year	M.	20 "	10 mg. T.	Reaction.
13	" in right neck	1 month	F.	10 "	5	"
14	" in left neck	3 years	F.	25 "	8	"
15	Enlarged glands	2 weeks	F.	3 "	2	Slight reaction.
16	Submental glands	3 years	F.	20 "	10 mg. K.	Reaction.
17	Enlarged glands in post-triangle of neck, right side	3 months	F.	30 "	7 mg. K.	"
18	Bunch of glands in left neck	2 weeks	F.	5 "	3 mg. K.	"
19	Suppurating glands in left neck	.....	M.	21 mos.	½, 1, 2	No reaction.
20	Glands on both sides of neck	9 months	F.	21 years	5, 7	Reaction.
21	Small suppurating glaud in right neck	.....	F.	18 "	5, 8	Reaction after 8 mg.
22	Glands in right neck	3 weeks	F.	12 "	3, 5	Slight reaction after 5 mg
23	Superficial glands, scars of former ones	2 months	F.	20 "	5, 7	Reaction.
24	Glands in right neck	2 years	F.	40 "	5	No reaction.
25	Small glands in neck	2 "	F.	8 "	2	Reaction.
26	Glands in right neck, some large	16 days	M.	23 "	7	"
27	Large mass of glands in the right neck	2 months	M.	Adult	10	No reaction.

[1] T. = Trudeau's tuberculin.

[2] K. = Koch's tuberculin.

TABLE III.—GENERAL CASES. 1898.

From the *Medical News*, July 9, 1898. "The Tuhereulin Test in Cervical Adenitis." by Edward O. Otis.

No.	Diagnosis.	Age.	Dose in mg.	Result.
1	Suppurating inguinal gland . . .	28	5	No reaction.
2	" " " . . .	34	5	" "
3	" " " both sides .	33	5	" "
4	" " " left side .	...	10	" "
5	" " " . . .	23	5	" "
6	" " " right side .	42	10	Probable reaction.
7	Enlarged gland, right axilla . . .	14	3	No reaction.
8	" " left axilla	40	3	Slight reaction.
9	Suspected tuberculous wrist, also complains of cough, and says she has spit blood	16	1	Reaction.
10	Indurated swelling of the right testicle, one month's duration ; no known cause	29	5	"
11	Indurated swelling of the left epididymis of three years' duration	24	5, 10	No reaction.
12	Swelling of right wrist, possibly tuberculosis	26	8	Reaction.
13	Arthritis of carpo-metacarpal joint of right thumb	23	5	No reaction.
14	Old sprain of ankle	21	5	" "
15	Necrosis of right thigh	23 mo	1	" "
16	Chronic laryngitis ; specific . . .	26	10	Moderate reaction.
17	Atrophic rhinitis ; pharyngitis sicca . .	21	5	Reaction.
18	" " " " . .	36	5	Moderate reaction.
19	Recent injury to side	60	5	No reaction.
20	Abdominal ascites, ten months . . .	19	5	Doubtful reaction.
21	Pulmonary tuberculosis, quite extensive consolidation ; tubercle bacilli in sputum	50	3, 5	No reaction.
22	Pulmonary tuberculosis. atrophic rhinitis ; tubercle bacilli in sputum . . .	24	5, 10	" "
23	Pulmonary tuberculosis, slight consolidation ; tubercle bacilli in sputum . .	24	5	Reaction.

TABLE IV.—GENERAL CASES. TUBERCULIN INJECTION. 1899.

No.	Diagnosis.	Age.	Dose in mg.	Result.
1	Clinical and physical evidence of pulmonary tuberculosis; no tubercle bacilli in sputum	11	7 T.[1]	Reaction.
2	Some clinical evidence of pulmonary tuberculosis; no tubercle bacilli in sputum	17	5, 7 K.[2]	No reaction.
3	Some clinical evidence of pulmonary tuberculosis	30	7 K.	" "
4	Clinical evidence of pulmon. tuberculosis; no tubercle bacilli in sputum	37	5	Reaction.
5	Clinical evidence of pulmon. tuberculosis; venereal sore 8 or 9 yrs ago	25	8, 10	Slight reaction.
6	Some clinical evidence of pulmonary tuberculosis; no tubercle bacilli in sputum	29	8	No reaction.
7	Slight clinical evidence of pulmon. tuberculosis; no tubercle bacilli in sputum	65	10	" "
8	Slight clinical evidence of pulmonary tuberculosis	12	3	Doubtful reaction.
9	Pulmonary tuberculosis in 2d stage; tubercle bacilli in sputum	29	5, 10	Reaction from 10 mg.
10	Pulmonary tuberculosis; tubercle bacilli in sputum	...	5, 10	No reaction.
11	Pulmon. tuberculosis, chronic laryngitis, tuber. bac. in sputum	42	10, 12	" "
11a	Sypbilis, adentitis in both sides of neck	35	5, 10	Marked reaction after 10 mg.; slight after 5 mg.
12	Syphilis	30	7 K.	No reaction.
13	"	...	7 K.	Reaction.
14	"	16	5 K.	No reaction.
15	"	48	10 K.	" "
16	"	23	10	Reaction.
17	"	22	10	No reaction.
18	Possible sypbilis	29	10	" "
19	Eularged anchylosed knee-joint	13	3, 5 K.	" "
20	Necrosis of pelvic bones	24	10 T. 10 K.	Reaction.
21	Chronic swelling and pain in rigbt knee, eight months' duration	27	10	No reaction.
22	Abscess in right hip; enlarged inguinal glands	14	3	" "
23	Cystic enlargement of rigbt testicle; pain and ill-defined mass in left iliac region from injury 1 year ago	25	10 T.	Reaction.
24	Enlarged, more or less acutely inflamed left elbow; syphilis 5½ yrs. ago	23	12 T.	"
25	Enlarged tbyroid glaud, 7 or 8 mos. duration	18	5	"
26	Chronic enlargement of tbe metacarpal bones of 1st and 2d finger of the left hand, 3 mouths' duration	37	5	"
27	Pleuritic effusion in right side, two weeks' duration	25	7	No reaction.
28	Cough, hæmoptysis, subsequently mitral insufficiency discovered	32	7	" "
29	Pulmonary tuberculosis; tubercle bacilli in sputum	24	7	Moderate reaction.
30	Cough for several montbs; purulent expectoration; no physical signs; no tubercle bacilli in sputum	18	7	No reaction.

[1] T = Trudeau's tuberculin.

[2] K. = Kocb's tuberculin.

DISCUSSION.

Dr. Langmaid: In the early days of tuberculin I saw a striking example of the injurious effects following its injection. Unknown to me, five or six patients in my service at the Massachusetts General Hospital were injected with the remedy, and shortly afterward, in passing through the ward, I noticed that something was amiss with the larynges of those patients. I had never before seen a larynx in such a terrible condition. My assistant then informed me that he had been induced to allow the injection of tuberculin to be made in a number of my patients. I immediately forbade its further use in my clinic. Since then, of course, tuberculin has been much modified.

I would like to ask Dr. Otis what the outcome was in the cases of cervical adenitis in which the tuberculin was used? Did the patients show any subsequent evidences of tuberculosis in the bones or in any other tissues of the body? Suppose he does get the evidences of tuberculosis in these cases of cervical adenitis, what does he expect the future course of these patients will be? What will be the outcome of these tubercular patients?

Dr. Otis: The primary object of this series of observations was in order to determine, if possible, what proportion of cases of enlarged glands in the neck were tuberculous in character. The only one I could find who had investigated this subject was Valland, and his evidence seemed to me unsatisfactory. The result of the injections in the various cases indicated in a measure the treatment. Valland and others are of the opinion that tuberculosis which manifests itself at first as a cervical adenitis frequently goes on to pulmonary tuberculosis.

I would like to ask Dr. Langmaid what dose of tuberculin was injected in the cases he referred to? I suspect it must have been the time of large doses, which we now know are dangerous.

Dr. Langmaid: I know nothing about it; I was not invited to be present at the injections.

Dr. Francis H. Williams, of Boston: I should like to ask Dr. Otis what his experience has been with the tuberculin test in private practice.

Dr. Roe: I should like to ask Dr. Otis what the final results of the tuberculin injections were? Whether they were harmful or not?

Dr. Otis stated that most of his observations were made on hospital patients, and the injections were not objected to. Recently I made an injection in a private patient, a young girl, who raised no objection to it. The injection was made in my office; no bad results followed, and no reaction occurred in this case, which seemed to settle happily a grave doubt. With regard to the future results in my cases I cannot speak positively, as my work was done in an ambulatory clinic and the patients were soon lost sight of.

SUBSEQUENT HISTORIES OF ARRESTED CASES OF PHTHISIS TREATED AT THE SHARON SANITARIUM.

By VINCENT Y. BOWDITCH, M.D.,
BOSTON.

My excuse for offering you a short paper upon a subject which I have often touched upon at these meetings, has both a personal and a professional nature; personal, because of my appreciation of the frequent and cordial encouragement I have received from members of this Society; professional because I think I am right in believing that the results obtained at the Sharon Sanitarium in the past eight years have justified the hope that incipient phthisis can be arrested and remain so in a good percentage of cases, even in our insalubrious New England climate, without the use of specific remedies other than the insistence upon hygienic and dietetic rules, such as can be best obtained in a well-regulated sanitarium for this purpose. It would be a trite and oft-repeated tale, were I simply to give results of work at this small institution after the brilliant and much larger records of those now well known throughout the medical world; but it must be remembered that the Sharon Sanitarium has been in certain particulars unique, in that it is situated near a large city, not far from the sea, at no special altitude, in our changeable and damp New England climate, with no pretence at the selection of a region specially suited for lung troubles other than what may be found in almost any hilly town; viz: a southern exposure on gravelly soil, as free as possible from dampness, and with protection by hills

and woods from the harshest winds. Hence, I feel I am justified in presenting the following histories, hoping that by so doing, I can still further stimulate the now rapidly-growing conviction that similar institutions are need throughout the country.

The title of my paper will explain my belief that it is of use, not only to know how lasting the effects of any one special method of treatment may be in a treacherous disease like consumption, but also to find out, if possible, the probable cause of recurrence of disease where it has unfortunately so happened, and thus learn by future effort to correct the mistakes made by our patients after they have passed from under our immediate supervision with the disease in an arrested state.

Since the opening of the Sanitarium, in February, 1891, up to September, 1898, thirty-six cases have been discharged as "arrested cases."

From the fact that the last two of these were at Sharon only a comparatively short time, and a year has not elapsed since their discharge, they are not included in this paper.

We will consider then only thirty-four cases, and, for the sake of convenience, I have divided them into four classes, as follows:

Class 1. Those who have died since leaving Sharon, 6;

Class 2. Those who have not been heard from lately, 1;

Class 3. Those who are living in active work, and apparently well, 24;

Class 4. Those who have had a slight return of their symptoms, but are still active, and, from outward appearances, well, after adopting similar methods to those adopted before, 3.

All of these cases, except two, had definite, although in some cases very slight, signs in the lungs, with varying histories of cough, expectoration, loss of flesh, malaise, and hæmoptysis. Although bacilli were found in the sputa of only 12 cases, yet the physical signs in the others were similar to those which, under unfavorable hygienic conditions, we usually find develop bacilli in the sputa later. Of the two cases in

which the signs in the lungs were not definite, one had shown symptoms of marked anæmia, debility, cough without expectoration, but was only considered a suspicious case. The other had bacilli in her sputa previous to her entrance. It will be impracticable to attempt to read the histories of these 34 cases, but in the published paper a tabulation has been resorted to for those who may wish to see the more detailed accounts. Of the six cases who have died, however, since their discharge, a short history will be instructive and of interest.

The first case, M. S., was one of rather advanced fibroid phthisis, for whom I had little hope other than amelioration of the most troublesome symptoms. There were marked signs of trouble in the right lung (dulness, râles, and harsh respiration), annoying cough, with profuse expectoration containing bacilli, and more or less fever. To my surprise, however, at the end of three months, the cough and expectoration had ceased, the temperature was normal, and the patient, against advice, resumed her former occupation ; but a few months later after her swallowing ammonia by mistake, having an attack of la grippe, and developing two tapeworms, the symptoms returned, and she died about two years ago after a slow decline.

The second case was a teacher, A. E., with definite signs (dulness and moist râles) in the top of the right lung, who at the end of five and a half months seemed by outward appearance to be well, the cough and expectoration having ceased. Entirely against urgent advice she left the sanitarium, resumed her teaching, and before the end of the year died suddenly from some cause which the physician in attendance was unable to explain. Without doubt, I think the disease had begun again in consequence of her resuming her laborious work in a badly ventilated school-room in the city.

The third case, L. McL., had had very marked signs in the right lung, which had improved decidedly previous to her entrance in October, 1892. At the end of four months all cough had disappeared, the faintest possible signs could be heard in the right apex only, and she left the sanitarium against advice,

and resumed very hard work as a seamstress, and was lost sight of. Indirectly the news of her death from some unknown cause was heard about a year later.

The fourth case, F. A. B., showed very clearly the influence of residence in a damp, unhygienic house. The patient had marked signs in the base of the right lung, with all the symptoms of tubercular disease, including bacilli in the sputa. After a stay of eleven months, she left, the picture of health, with the disease apparently arrested. For eight months she remained in a healthful sunny home in the country, apparently perfectly well. Finally, against my most urgent advice to herself and her family, she returned to her home near the sea, with exactly the same conditions of dampness and lack of sun. In three weeks she suddenly developed an acute process in her lung, and went rapidly down hill and died not long afterward.

The fifth case, S. C., was of special interest because of the fact that she came at two different times to the sanitarium (1893) with distinct evidence of tubercular disease in the lungs (bacilli, râles, etc.) and twice all the symptoms disappeared in a period of twenty-one months. The patient had to undergo, twice after leaving the sanitarium, severe uterine operations. Previous to the second, at the City Hospital, Dr. A. L. Mason examined her lungs (1895), and could find no trace of disease. The patient suddenly died after the operation, from unknown cause, and no autopsy was allowed. That the pulmonary trouble had been arrested there was not the least doubt, however. Whether the ovarian and uterine disease was of tubercular nature or not was not known.

The sixth case, M. J. T., entered in November, 1894, with cough and questionable signs at the left apex, but having no bacilli in the sputa. The cough and signs disappeared completely, and the patient insisted upon leaving, against urgent advice, resuming her previous hard, confining work as a domestic. A few months later she appeared again with all the signs of tuberculosis in the left lung, including bacilli in the sputa, and went rapidly down hill and died a few months later.

As an offset to these cases, allow me to mention two which show what can be done when different methods are adopted after leaving the sanitarium. H. Q. entered in July, 1893, with the usual signs of incipient phthisis in the right apex and with bacilli in her sputa, having been told by a physician in Ireland that she probably never would recover. For several months the cough and tuberculous sputa persisted, but finally ceased, and fourteen months later she left the sanitarium a magnificent specimen of health, having gained over thirty pounds in weight; the râles and bronchial respiration in the right apex being supplanted by a somewhat harsh respiration only. She was fortunate in obtaining a position as domestic in a kind family in the country, where her friends made her exercise frequently in the open air, and during the past four and a half years she has not only made a visit to her old home in Ireland, where she joyously told me she "showed herself to the doctor who had told her she had to die," but she frequently visits the sanitarium "to brace up the girls and show them how well she got."

Another case, G. M., contracted phthisis, doubtless by nursing a brother dying with that disease. Every member of her immediate family, five in number, had died of phthisis. Râles in both apices and bacilli were found upon entrance, in May, 1896. After a stay of fifteen and a half months, all traces of the disease, except some lack of tone in the apices, disappeared. She left the sanitarium the picture of health, having had no cough or sputa for months. We obtained positions for her in Florida during the succeeding winters, and she has returned to Boston in the summers. Up to the present time she has had no symptoms of the return of the disease, it being now over two and a half years since the cough and expectoration ceased. She now lives in Florida.

I could cite the cases of others, similar in character, who have been away from the sanitarium even longer than those just mentioned, one having been, seven years ago, discharged who, with slight signs at both apices, cough, hæmoptysis, etc.,

is now, from all accounts, perfectly well, but I must refer you to the tabulated results instead.

In every case strenuous effort is made to get the patients not to return to the same conditions under which the disease developed. Being all of them more or less dependent upon their own efforts, the task is not always an easy one, but a little energy in this direction will often accomplish the desired end.

I am aware, as I have said in my two previous papers giving the results of treatment at the sanitarium,[1] that some may challenge the results because of the failure to find bacilli in some of the reported cases. I can only say in reply that the patients have been carefully observed, and none taken where the symptoms were not such as experience teaches us usually develop the more definite signs later. I feel, too, that I can say with justice that although the work accomplished at the sanitarium is comparatively small, yet, from that very fact, observation of individual cases has been made possible to prove what can be done on a larger scale.

A word as to methods pursued at the sanitarium:

Although two or three of the so-called "specifics" have been resorted to occasionally, the chief factors in the treatment have been the insistence upon fresh air, judicious exercise, and good wholesome food, medicines having been used somewhat sparingly, and as mild adjuncts for general treatment only.

What then can we learn from these observations? Not only that a larger percentage of cases of incipient phthisis can be arrested by sanitarium treatment than is possible by treating patients in our offices or in their homes, but that this can be done near all our great cities, and that lasting results can be accomplished when patients are not allowed to go back to former unhealthful conditions.

That I may live to see the day when similar institutions are

[1] Three Years' Experience with Sanitarium Treatment of Pulmonary Diseases, etc., Boston Medical and Surgical Journal, July 12 and 19, 1894. The Treatment of Phthisis in Sanitaria near Our Homes, Transactions of the Massachusetts Medical Society, 1896.

not the exception, but the rule, near every large city and town throughout the country, is my earnest desire.

Massachusetts, I am pround to say, has been the first to take the step in this direction by establishing the State Hospital (or Sanitarium, as I prefer to call it) for Consumptives, at Rutland, Mass. New York will soon have a similar State institution, and I cannot think that other States will be slow to follow their example.

TABULATION OF THIRTY-FOUR ARRESTED CASES OF PHTHISIS TREATED AT THE SHARON SANITARIUM.

No.	Name, and time of entrance.	General symptoms.	Stage of disease.	Part affected.	Length of stay in Sanitarium.	Time since discharge.	Bacilli in sputa.	Result.	Remarks.
1	M. S. Feb. 1891	Cough for three or four years with sputa.	Somewhat advanced.	Upper portion of right, and to less degree in upper left; (dulness, harsh respirat'n, and few râles).	3 mos.	.....	Yes	Cessation of cough and sputa and fever.	Resumed former occupation of typewriting; reappearance of symptoms; gradual failure. Died, 1897.
2	A. E. March, 1891	Cough for several months with slight hemorrhages.	Incipient stage, but well marked.	Slight dulness and moist râles in right upper chest.	5 mos.	.....	Yes	Cessation of cough and fever.	Resumed teaching; remained fairly well until Dec. 1892. Died suddenly, Dec. 1st; cause unknown; probable return of disease.
3	J. W. June, 1891	Cough for a year or more; had been treated with Koch's tuberculin.	Somewhat advanced, but had nearly lost cough before entrance.	Upper portions of both lungs; larynx had healed ulcerations; râles dry in both apices with harsh respiration.	3 mos.	7 yrs.	No	Cessation of cough; cicatrices in larynx; voice hoarse.	Still living in Boston, and at last accounts very well as far as pulmonary symptoms are concerned.
4	J. R. W. Jnly, 1891	Bad family history; cough hæmoptysis for several months before entrance.	Incipient.	Clicking râles in both apices, with slight dulness.	1 year.	7 yrs.	No	Cessation of all active symptoms.	Since married, and has a healthy child; living in Newton, and very well.
5	N. M. B. Oct. 1891	Cough, mucopurulent sputa; loss of flesh and strength for several months.	Incipient.	Slight signs in both apices. (Râles with slight dulness.)	6 mos.	7 yrs.	No	Cessation of cough and sputa; gain of 28 lbs.	Sent by Dr. J. Ellery Steadman. Has lived in Dorchester since in a different house; occasional colds, but, as a rule, very well. Patient examined in 1899, and nothing new had developed.
6	G. E. M. Nov. 1891	Cough, sputa, hæmoptysis, loss of flesh and strength for several months previous.	Incipient.	Slight dulness in left apex.	4 mos.	7 yrs.	No	Cessation of cough and hæmoptysis; gain of 8 lbs.	Sent by Dr. Sheldon, of Lynn. Has remained well since; married later.

7	J.A.M.D. April, 1892	Cough, little sputa, hæmoptysis, loss of flesh and strength for months.	Incipient.	Faiut râles in both apices, with slight dulness in left front and right back at top.	5½ mos.	6½ yrs	No	Cessation of congh and sputa; gain of 8½ lbs.	Went home against advice, bnt later went to Canada. Has married, and had septicæmia after childbirth aud typhoid fever; recovered from both. Once had return of hæmoptysis. When last seen, râles were heard in left apex, but patient was feeling very well. Living near Boston.
8	M. L. April, 1892	Cough, scanty sputa, loss of flesh and strength.	Incipient.	Faint râles in right apex.	5½ mos.	6½ yrs.	No	Cessation of cough and sputa; felt perfectly well.	Resumed former hard occnpation of dressmaking against advice, but has remained very well until lately, when slight hæmoptysis and persistence of râles persuad'd her to cease work. Now doing well at Rutland, Mass.
9	A. R. L. May, 1892	Cough, scanty sputa, malaise, and loss of flesh; bad family history.	Incipient.	Râles; slight dnlness at left apex; later in right apex and in lower right back, with dulness.	2 years	5 yrs.	No	Cessation of cough and other abnormal symptoms; gradual clearing of râles at left apex; few dry râles in right apex; gain 27 lbs.	Sent by Dr. C. Ellery Steadman. One sister died, just before patient came to Sanitarium, of phthisis, and later an adopted sister and brother. Patient has remained perfectly well; exercises in open air every day.
10	L. A. July, 1892	Cough. sputa, loss of flesh and strength; anæmia for months, hæmoptysis.	Incipient.	Râles at right apex.	2 years.	5 yrs.	Yes	Cessation of all abnormal symptoms; gain 33 lbs.	Perfectly well since; has been in active work, in and near Boston, nnrsing.
11	L. H. Oct. 1892	Cough, hæmoptysis frequently, malaise and loss of flesh for months.	Incipient.	Clicking râles in both apices.	2⅔ years.	4 yrs.	Yes	Cessation of abnormal symptoms; clicking râles not so prominent; gain of 17 lbs.	Persistence of cough for months after entrance; gradual improvement. Returned to home near the sea against advice, bnt kept up sanitarium methods, and has remained perfectly well since.
12	L. M. L. Oct. 1892	Cough, profuse sputa; great loss of flesh aud strength with marked amelioration of signs and symptoms beforeentrance.	Rather advanced signs in one lung, with decided improvement.	Slight dulness aud dry râles in right apex; râles in right lower axillary region.	4 mos.	.....	No	Cessation of cough and spnta; felt better than for years before; Disappearance of râles in right apex.	Resumed hard work for several months. Lost sight of, but death occnrred (1893?), from some unknown canse, suddeuly.

No.	Name, and time of entrance.	General symptoms.	Stage of disease.	Part affected.	Length of stay in Sanitarium.	Time since discharge.	Bacilli in sputa.	Result.	Remarks.
13	F. A. B. Jan. 1893	Cough, sputa, malaise, loss of flesh for several months.	Somewhat advanced.	Râles in right apex; dulness, bronchial respiration and râles in right base.	11 mos.	.....	Yes	Cessation of cough and sputa; felt perfectly well.	Remained well for 8 months while living in a sunny, dry house in the country, away from the sea. Against advice returned home later to damp house near sea. Acute process developed in three weeks, and symptoms all reappeared. Died, July, 1895.
14	S. C. Jan. 1893	Cough, sputa, malaise, loss of of flesh, etc., for several months.	Incipient.	Slight dulness in right apex, with faint, indistinct crumpling râles.	1 year, 9½ mos.	.....	Yes	Cessation of cough and expectoration; apparently well, except for uterine difficulty.	At end of nine months cough and expectoration had ceased. Went to Vincent Hospital for uterine treatment. Cough returned, and patient re-entered Sanitarium; remained a year, with entire disappearance of pulmonary symptoms. Again obliged to enter hospital for operation; died from shock, Dec. 1896.
15	H. Q. July, 1893	Cough, sputa, malaise, loss of flesh and strength for several months.	Incipient.	Slight dulness, in middle of right back; moist râles.	14 mos.	4¾ yrs.	Yes	Entire cessation of cough and expectoration at the end of 8 months; gain of 37 lbs.	Left feeling perfectly well; has lived in country since, with no return of symptoms.
16	M. N. S. Oct. 1893	Cough, hæmoptysis, loss of flesh for months.	Incipient.	Slight dulness right apex; broncho- vesicular respiration.	5½ mos.	5 yrs.	No	Cessation of symptoms.	Went to live in South. California; not heard from since short time after departure, when apparently well.
17	M. W. Jan. 1894	Cough, sputa, malaise, loss of flesh, hæmoptysis.	Somewhat advanced.	Dulness with râles over most of right lung; few at left apex.	17 mos.	4 yrs.	Yes	Entire cessation of cough, sputa, etc., for months before leaving Sanitarium.	Living now in town of Sharon, without definite return of pulmonary symptoms.
18	R. B. May, 1894	Cough, sputa, malaise, etc., for months.	Incipient.	Dulness, jerky respiration and tenderness right apex, slight fever.	13¼ mos.	4 yrs.	No	Cessation of cough and fever.	Still living in suburbs of Boston. No return of pulmonary symptoms.

19	E. V. M. July, 1894	Cough, loss of flesh, expectoration for months; had family history.	Incipient.	Dulness both apices with faint explosion of râles; tubular respiration.	21¼ mos.	3½ yrs.	Yes	Entire cessation of cough, sputa, and fever for months previous to discharge; picture of health; gain of 15 lbs.	Went to Minnesota; returned to Boston two years ago. Six months ago had no cough or expectoration. Somewhat run down by uterine difficulty.
20	M. J. T. Nov. 1894	Cough two months; expectoration, loss of flesh.	Incipient.	Questionable sign left apex.	2¾ mos.	.....	No	Entire cessation of cough and sputa.	Patient left against advice; resumed hard work; symptoms reappeared; rapid progress of disease, and she died a few months later.
21	L. E. W. Jan. 1895	Cough, slight sputa, loss of flesh and strength; anæmia for several months.	Incipient.	Harsh respiration in both apices in back; faint, fine, dry crepitation near spine of left scapula.	4 mos.	4 yrs.	No	Disappearance of all abnormal symptoms except prolonged expiration at both apices.	A few months ago reported she had been perfectly well since.
22	H. G. H. Jan. 1895	Loss of flesh, constant malaise; previous cough with slight expectoration.	Incipient.	Previous to entrance Dr. G. G. Sears found moist râles in left apex; at entrance, decided bronchovesicular respiration in both apices, with increased voice front and back.	4 mos.	4 yrs.	No	Cessation of abnormal symptoms; slight harshness of respiration in both apices.	Patient since lost sight of, but a year after discharge was apparently well.
23	D. S. May, 1895	Cough with slight expectoration for many months; loss of flesh, etc.	Incipient, but well-marked.	Dulness in left apex; râles over all of left; a few at top of right.	17 mos.	4 yrs.	Yes	Cessation of cough and sputa; very marked improvement in all conditions; dry râles in l. top persisting; gain of 21 lbs.	Still living and in very good health in country suburbs of Boston. Physical examination in March, 1899, showed no increase of pulmouary signs.
24	M. P. B. May, 1896	Cough, sputa, malaise, loss of flesh, etc., several months.	Incipient.	Dulness and faint moist râles in right apex.	12¾ mos.	3 yrs.	Yes	Cessation of cough aud sputa; general condition excelleut; gain of 18 lbs.	Went to live in country in northern New York State. A few months ago wrote that she never felt better.

No.	Name, and time of entrance.	General symptoms.	Stage of disease.	Part affected.	Length of stay in Sanitarium.	Time since discharge.	Bacilli in sputa.	Result.	Remarks.
25	B. F. M. May, 1896	Cough, sputa, loss of flesh and strength three months; father, mother, and two brothers died of phthisis.	Incipient.	Slight dulness in right apex, expiration prolonged; faint râles in left apex.	15½ mos.	1¾ yrs.	Yes	Disappearance of all abnormal symptoms except slight dry crepitation in lower left axillary region several months previous to discharge.	Living in Florida now, and perfectly well since discharge. Case remarkable, considering that every member of immediate family died of phthisis.
26	A. G. C. June, 1896	Cough, slight sputa, loss of flesh, strength, anæmia some time before entrance.	Suspected Incipient only.	Respiration broncho-vesicular.	6 weeks.	1 year, 10 mos.	No	Disappearance of abnormal symptoms.	This case is the only one which was doubtful, and yet had many of the symptoms of cases which if allowed to go on usually develop pulmonary disease.
27	N. A. C. July, 1896	Cough, sputa, loss of flesh and strength for months; slight hæmoptysis.	Incipient.	Respiratory murmur increased, and whispered bronchophony; ? râles at left apex.	12¾ mos.	1 year, 10 mos.	Once doubtful.	Disappearance of abnormal signs, except slight change from normal in apices.	Has remained well since, not having resumed confining occupation. Respiration at right apex, when examined some months ago, decidedly broncho-vesicular, but patient was perfectly well.
28	A. K. July, 1896	Cough, sputa, loss of flesh; malaise, etc., for several months.	Incipient, but well-marked.	Râles, slight dulness in both apices.	9 mos.	2 yrs.	Yes	Improvement in physical signs and general symptoms; discharged as "much improved."	Patient discharged not as an arrested case, but was obliged to leave. Afterward lived in the country doing only light work. Two months ago wrote that after keeping up sanitarium rules she had had no cough or expectoration for over a year, and felt perfectly well. The case has been therefore classed as "arrested."

29	M. E. A. Dec. 1896	Cough, slight expectoration, loss of flesh and strength for several months.	Incipient.	Some dulness and crepitation in lower left left chest.	9 mos.	2 yrs.	No	Disappearance of abnormal symptoms and felt perfectly well.	Resumed former occupation with confining work; slight return of symptoms later. Re-entered Sanitarium, where she now is, without definite symptoms in chest, with slight cough without expectoration.
30	S. K. C. March, 1897	Severe hæmoptysis as a child; cough over a year; loss of flesh and strength.	Incipient.	Harsh respiration in both apices, especially on right, whispered bronchophony, coarse crepitation in lower right.	8 mos.	1½ yrs.	No	Disappearance of abnor. symptoms; prolonged expiration in right top; faint dry rales in right base; gain of 31¼ lbs.	Resumed, against advice, work in restaurant in Boston; at last accounts working hard, not feeling very strong.
31	J. E. P. May, 1897	Slight cough for two years; slight expectoration, malaise, loss of flesh.	Incipient.	Faint clicks in both apices; respiration broncho-vesicular in right apex behind.	3½ mos.	1¾ yrs.	No	Faint clicks in left apex; disappearance of cough and sputa; patient feeling well.	Patient left against advice, but changed occupation, and has remained well since.
32	J. A. M. Oct. 1897	Cough, expectoration, great loss of strength and flesh.	Incipient.	Coarse, faint rales, slight dulness in lower left axillary region and back; faint bronchial respiration.	11½ mos	9 mos.	Yes	Complete cessation of all abnormal symptoms; slight, dry rales heard in lower left chest; patient feeling and looking perfectly strong and well.	Patient at first prostrated and confined to bed for weeks after nursing husband who died of phthisis. Now living in the country, and has remained well since, with exception of slight cold lately.
33	C. B. M. Oct. 1897	Slight cough, occasional bloody expectoration, loss of weight and strength for months.	Incipient.	Faint rales in both apices.	5½ mos.	1 year.	No	Cessation of all abnormal symptoms.	Patient now living in the country, and writes she has no cough and feels better than for years.
34	Mrs. C. May, 1897	Cough and expectoration for several weeks; loss of weight and strength.	Incipient.	No definite signs in chest, but just previous to entrance bacilli found in sputa.	3½ mos.	1¾ yrs.	Yes	Cessation of cough, and marked improvement in general condition.	Left Sanitarium to go to Maine woods; since then has remained perfectly well.

DISCUSSION.

Dr. J. Edward Stubbert, of Liberty, N. Y.: I was very much interested in the paper of Dr. Bowditch, bearing, as it does, upon the curability of incipient cases of phthisis, and emphasizing the remarks made by another member of the Association in regard to the importance of nourishment *versus* climatic influence. I think the results obtained by Dr. Bowditch are very encouraging, especially when we bear in mind the fact that the work was carried on in a climate which is not considered particularly salubrious.

The temporary absence of bacilli in the sputum of tuberculous patients is well known. I have in mind at present a patient of Dr. Janeway's who has a large cavity in the upper portion of the left lung and a fibrous condition of the lower portion of the lung, in whose sputum the bacilli were absent for five weeks, although they were frequently searched for by three different examiners. It is not uncommon, in my experience, in incipient cases, to get physical signs without being able to detect the presence of bacilli. Even in sanitariums special attention must be given to the complications of pulmonary tuberculosis; otherwise such complications will handicap the patient and interfere with his recovery, even in the best of climates.

Dr. Newton: About half a year ago my friend, Dr. Stubbert, sent a girl from the Liberty Sanitarium to her home at Montclair to die. She was suffering from advanced pulmonary tuberculosis. Since her return home, in spite of the fact that she has been living in very unhygienic surroundings in a tenement-house, she has gained twelve pounds in weight, and apparently is going to get well in spite of us.

Dr. Francis H. Williams, of Boston: I should like to ask Dr. Bowditch what his experience has been with the treatment of cases of pulmonary tuberculosis in the wards of a general hospital during the limited time they are kept there as compared with the sanitarium cases. I have frequently been much gratified to see patients with pulmonary tuberculosis improve in a general hospital in a remarkable manner simply on the food and care such a hospital affords. A comparison between hospital and sanitarium cases in similar stages of the disease would be instructive.

Dr. Bowditch: Poorly nourished patients suffering from pulmonary tuberculosis who are admitted to a general hospital often improve temporarily, which is doubtless due to the better food and care they receive, but they cannot get the treatment there that they receive in a special hospital. They cannot get the same amount of fresh air that they would in a properly regulated sanitarium. If we could treat this class of patients by themselves in hospitals espe-

cially constructed for their use we could do more for them, even in the city.

EDWARD R. BALDWIN, of Saranac Lake: I would like to say that the authorities at the Adirondack Sanitarium are in constant correspondence with 115 patients who have been discharged during the past ten years. Most of these are in fairly good health. Some have relapsed slightly, but the majority are able to reside at their old homes and resume their usual occupations.

DR. STUBBERT: I would like to ask what distinction Dr. Bowditch makes between arrested cases and apparently cured cases.

DR. BOWDITCH: I simply adopt the term "arrested" because I regard it as a more appropriate term to apply to these cases than 'cured," unless a long period has elapsed without a return of the symptoms. I apply it to every case where definite tuberculous symptoms have existed, when cough, expectoration and fever have ceased and the general condition signifies a return to normal conditions, even though abnormal physical signs may persist indefinitely.

RÖNTGEN RAY EXAMINATIONS IN INCIPIENT PULMONARY TUBERCULOSIS.

By FRANCIS H. WILLIAMS, M.D.,
BOSTON, MASS.

During the past three years I have seen about 275 cases of pulmonary tuberculosis, and I have examined about 165 of them by means of the X-rays.[1] In making these X-ray examinations my attention was early directed to the darkened apex and the shortened excursion of the diaphragm, which were signs common to nearly all of these cases, and it seemed to me that these appearances warranted further study with reference to their usefulness in detecting pulmonary tuberculosis in the incipient stage.[2] It is this part of my work with the X-rays that I am to have the pleasure of laying before you this morning, or, rather, such portions of it as may be presented in the few minutes at my disposal. I will first ask your attention to a few incipient cases, in choosing which I have taken only those where the diagnosis was confirmed by finding the tubercle bacilli or by reaction to the tuberculin test. I have divided these cases, for convenience, into two groups. *First*, those with slight physical signs, but no râles except in Case III. *Second*, those without physical signs—that is, whose lungs had been found normal by auscultation and percussion. In most of these an X-ray examination was made for the purpose of determining the size of the heart, and not because the lungs were suspected, but during the course of

[1] The static machine which I now use for X-ray examinations has four revolving plates 6 feet in diameter and four fixed plates 6 feet 4 inches in diameter.

[2] In this paper I have presented more fully questions which I first brought out in medical meetings in 1896 and published in the Medical and Surgical Reports of the Boston City Hospital, January, 1897.

this examination the lungs were found to be abnormal. The patients in both groups were all hospital patients.

First Group. I described three cases in the paper presented to this Association at the last meeting, which were sufficiently typical of the first group, namely, that with slight physical signs, in which the X-ray examination was of assistance. I will cite here seven more such cases to add to the evidence brought before you at that time. Most of the patients referred to are doing well.

Case I.—F. J. M., aged twenty-four years. Diagnosis: acute articular rheumatism, cardiac. Admitted to the Boston City Hospital, December 2, 1898. Family history negative. Personal history: Was in the hospital with acute articular rheumatism in June, 1898, for two weeks, and again in July and August, 1898, for four weeks. Lungs were then normal, temperature such as might be expected with rheumatism, not suggestive of tubercuIosis. After his discharge from the hospital in August his ankle continued to trouble him, but he kept at work until the middle of November. Then both ankles were painful, and the right wrist was also affected. December 2, 1898, says he has felt feverish and slept poorly, on account of pain; no headache, no cough, appetite good. Physical examination: Well-developed and well-nourished man, general condition good. Pulse 76. Lungs: Resonance and respiratory sounds normal.

December 17th, X-ray examination was made in order to determine the size of the heart. Heart shows some enlargement, both to the right and left. I noticed while examining this organ that the right lung, from the apex as far as the third rib, was darker than normal, and the excursion of the diaphragm was diminished on this side, being two and one-quarter inches on the right side and three inches on the left side. [Let me interpolate here that in forty-five normal adults that I examined with the fluoroscope, thirty-one men and fourteen women, the average excursion of the diaphragm on the left side was two and one-half inches, on the right side about one-eighth of an inch more. In younger adults

the average excursion was somewhat greater than among the older.]

On the same day the physical examination was made after X-ray examination: Expiration is somewhat more marked in the right apex in front. Tactile fremitus is slightly increased in the right back between the scapula and the vertebral column. Patient when asked if he had not lost in weight during the past few months, thought that he had. Three attacks of rheumatism would account for this. December 22d, one milligramme of tuberculin was given; no reaction. December 27th, three milligrammes of tuberculin were given; no reaction. December 30th, five milligrammes of tuberculin were given; reaction well marked. Temperature 102.5° F., with malaise. December 30th, morning and evening temperature for three weeks had been 98° to 99° F. No cough at any time.

January 2, 1899, prolonged expiration, slight increase in tactile fremitus in the right back between the scapula and spine, opposite the spine of the scapula. Discharged. His appearance at this time was that of a well man. Physical examination of lungs as on December 17th.

In March, 1899, this patient had another attack, and returned to the City Hospital. He was under Dr. V. Y. Bowditch's care. Diagnosis: rheumatism. With Dr. Bowditch's permission I insert here the note he made of his physical examination of the lungs: "Percussion note is slightly high in pitch, and slight dulness at right apex to the third rib in front and spine of scapula behind. No change in respiratory murmur and no râles. Resonance and respiration good over balance of chest."

In March, 1899, I made another *X-ray examination* and found the same darkened area of the lung and shortened excursion of the diaphragm as on December 17, 1898.

Case II.—Michael H., aged thirty-six years. Entered the hospital March 7, 1898, with acute articular rheumatism, which began five weeks before entrance, with swelling in the

knee, which extended to one hip, then to the other, then to the left ankle. Ten days before entrance both hands became red, swollen, and painful; five days before started again in the knee, right hip, right hand and shoulder. Physical examination of the lungs showed good resonance and respiration throughout. On March 10th the left wrist was red and painful. On March 18th this condition improved under salicylates. Practically well.

March 8th, X-ray examination. This was made to determine the size of the heart, as I was then making X-ray examinations of this organ in all cases of acute rheumatism. The apex of the right lung was found to be shaded as compared with the left lung; excursion of the diaphragm on the right side two and one-quarter inches; on the left side three and one-quarter inches.

After this examination auscultation and percussion showed slight dulness on the right side from the apex to the second rib. Tactile fremitus increased, expiration prolonged.

14*th. Second X-ray examination* pointed to the right lung as the seat of the trouble, as before.

One milligramme of tuberculin given, later two milligrammes given, finally four given. Patient reacted to this last dose. Discharged April 7, 1898. January, 1899, inquiry was made in regard to the patient, and he was stated to be in very poor health.

In May, 1899, he returned to the out-patient department, and Dr. Bartol kindly reported to me that he found no physical signs in the lungs.

Case III. is inserted because I was able to watch its progress as it ran nearly the whole of its very acute course in four months, and largely in my service. It, as well as the other cases that I have seen, shows that in acute tuberculosis we may be warned of the rapid progress of the disease by making two or three X-ray examinations at intervals of a week or so, and thus recognize the serious character of the attack.

Case III.—Sarah H., aged twenty-three years. Entered

the Boston City Hospital, April 20, 1896, with acute articular rheumatism, first in the right ankle, then in the left ankle and right elbow. She had had cough, with bloody expectoration, for two weeks. The respiration and resonance in both lungs were good throughout. Heart by percussion seemed enlarged to the right.

April 24th. X-ray examination made to determine the size of the heart. This showed that the heart was not enlarged to the right, but there were signs of tuberculosis on the left side, namely, the left apex was shaded and the excursion of the diaphragm on the left side was two inches, while on the right side the apex was clear and the excursion of the diaphragm was two and five-eighths inches.

Physical examination, after X-ray examination, showed many crackling râles over lower part of left front and axilla.

On May 6th nearly the whole of the left lung was shaded, and the excursion of the diaphragm was shortened by one-half an inch and moved one and one-half inches only. The apex of the right lung was now also darker than normal, and the excursion of the diaphragm was two and one-eighth inches. On May 12th the left lung was still darker than it had been, and the excursion of the diaphragm was reduced to one inch. Patient up and about. On May 17th the excursion of the diaphragm was reduced still further, namely, to one-half inch on the left side and to one and one-half inches on the right side.

The appearances seen in the X-ray examination were confirmed by finding tubercle bacilli. Patient died in July, 1898.

Case IV.—Tofilus K., aged thirty-five years. Entered the hospital, January 11, 1899, with acute articular rheumatism. Physical examination: Increase of vocal resonance and fremitus, and marked whispered bronchophony in the right apex behind to an inch below the spine of the scapula. Over this area respiration was roughened and expiration much prolonged, but there were no râles and no dulness.

X-ray examination was made in order to determine the

size of the heart. Upper half of the right lung was shaded; excursion of the diaphragm on this side one and one-half inches; on left side two inches. Tuberculin was given; reaction. Temperature 102° F.

CASE V.—Dennis H. F., aged twenty-eight years, entered the hospital, September 6, 1898, with malaria. While in the hospital he had some cough and bloody expectoration. Physical examination: Fremitus increased over the whole of the right side, especially at the apex; voice sounds also more marked on right side; no râles heard anywhere. No tubercle bacilli found in the sputa.

X-ray examination. Left side less clear than right. Excursion of the diaphragm on the left side five-eighths of an inch, on the right side one and five-eighths inches. Three milligrammes of tuberculin were given after the temperature had been running from normal to 99° F. for some time. Temperature rose in eighteen hours to 103° F.

CASE VI.—F. A., aged nine years, entered the hospital, November 14, 1898, with slight fever, thought to be typhoid. Physical examination of the lungs showed an increase in vocal resonance and fremitus, and whispered bronchophony in the right apex front and back. Lungs otherwise normal. No change in percussion note, no râles heard.

November 22*d*, *X-ray examination.* Upper portion of the right lung is not as bright as the left. Excursion of the diaphragm shorter and higher up on the right side than on the left, one and one-quarter inches on the right and two inches on the left.

Three milligrammes of tuberculin were given; reaction. Temperature 102.5° F.

February 27, 1899, *second X-ray examination.* Darkened right apex. Excursion of the diaphragm on the right side one and one-quarter inches; on the left side one and seven-eighths inches.

Some dulness to percussion over right apex.

Case VII.—Sadie McD., aged twenty-four years, entered the hospital, April 6th, 1898. Question of typhoid fever; temperature varies from 100° to 104° F. It is irregular. Physical examination of the lungs at entrance: Resonance somewhat diminished over fronts of both chests below clavicles, otherwise good. Respiration good throughout. Widal's test negative.

X-ray examination showed a well-marked shading on the left side as compared with the right. Excursion of diaphragm on the left side one and three-quarter inches, on the right two and one-eighth inches. After the X-ray examination had been made the chest was again percussed, and it was thought that the percussion note on the left side was less resonant than on the right side. One-half milligramme of tuberculin was given, then one milligramme, then three milligrammes were given; reaction.

It will be noticed that four cases in this group had acute articular rheumatism. I have been struck with the frequent association of rheumatism and tuberculosis in these and other similar cases.

Second Group (five cases), namely, those without physical signs, that is whose lungs were normal by auseultation and percussion.

Case I.—Mary C., aged twenty-five years. Hysteria. Says she vomited about a quart of blood about three days ago and again this morning. Did not raise any blood while she was in the hospital. March 20th, physical examination of lungs: Resonance and respiration good.

April 1st, X-ray examination. Excursion of the diaphragm on the right side two and one-quarter inches, on the left side three and one-quarter inches. Three subsequent X-ray examinations showed a shortened excursion on the right side. Patient reacted to tuberculin. Tempearture 105° F.

Case II.—Bertha M., aged twenty years. February 18, 1899, physical examination of lungs: Resonance and respiration good.

Two *X-ray examinations* showed a darkened apex and a shortened excursion of the diaphragm on the right side. Patient reacted to ten milligrammes of tuberculin. Temperature 101.2° F.; malaise.

CASE III.—M. T., aged twenty years, entered the hospital, April 21, 1897, for phlebitis in the right leg. Physical examination of lungs showed good resonance and respiration throughout.

May 12th, X-ray examination. Right lung shaded. Excursion of the diaphragm on the right side one and seven-eighths inches, on the left side two and seven eighths inches. May 22d reacted to five milligrammes of tuberculin. Temperature 104.8° F.

27th. Physical examination shows nothing in the lung. Patient has no cough, is well developed and well nourished; no symptoms pointing to trouble in the lungs.

June 12th. Several careful physical examinations of the lungs made during the past month gave no evidence of tuberculous process. Resonance and respiration were good. Discharged.

CASE IV.—Julia D., aged twenty years, entered the hospital, March 13, 1899, for what was thought might be typhoid fever. No loss in weight. Unusually well nourished and well developed. Physical examination of the lungs shows resonance good throughout, respiration clear and distinct.

X-ray examination. Right apex to about the third rib is shaded. Excursion of the diaphragm on the right side one and five-eighths inches, on the left side two and three-quarter inches. March 25th, five milligrammes of tuberculin were given; reaction. Temperature 103° F.

April 14th. Jerky respiration on the right, but nothing that can be considered abnormal.

CASE V.—Joseph M., aged nineteen years, entered the hospital, February 6, 1897. Physical examination of the lungs showed good resonance and respiration throughout.

March 12*th* an *X-ray examination* was made in order to examine the heart. Right lung darker from the apex to the third rib. Excursion of the diaphragm two inches on the right side, two and one-half on the left side. No signs by auscultation and percussion at this time.

21*st*, *second X-ray examination.* Whole of the right side darker than the left. Diaphragm seen only in full inspiration on the right side; excursion on left side two and one-half inches.

April 19*th*. Physical examination: Some dulness of upper part of the right chest. Respiration diminished. No râles. Vocal and tactile fremitus very slightly increased. Resonance and respiration good over the left chest, both front and back.

25*th*. Physical examination the same as on April 19th.

27*th*, *third X-ray examination.* Right side darker than normal throughout, and on the left side darker from the apex to the upper border of the fourth rib. Excursion of diaphragm not seen on the right side; on the left side it is one and three-quarter inches.

27*th*. Physical examination: No signs on the left side at this time by auscultation and percussion.

Patient died on June 4th. No tubercle bacilli were found at any time, though frequent examinations were made, but on April 13th he reacted strongly to five milligrammes of tuberculin; marked malaise; temperature 101.5° F.

We see in this second group (of five cases), as well as in similar cases which I have presented elsewhere, that the X-ray examination gave warning of tuberculosis before any departure from the normal was obtained by auscultation and percussion. Let me indicate one or two reasons why we may thus detect increase in density in cases where it is not found by auscultation and percussion. *First*, if the density is much below the surface of the lung, as in central pneumonia, its presence is not recognized by auscultation and percussion, but a shadow of the consolidated portion of the lung is cast

on the fluorescent screen as surely as if the pneumonic process were near the surface. This I have observed in several cases. *Second*, judging from the following tests, made at the hospital in 1896 with lungs taken from post-mortem examinations, a shadow may be thrown on the fluorescent screen by a slighter degree of congestion than might be detected by auscultation and percussion. For these two reasons the X-ray examination may be the more delicate test under suitable conditions.

Tests. Two feet above a Crookes tube I placed a piece of cardboard on which I set a thin, vulcanite cup—both the cardboard and the vulcanite offered little resistance to the rays —and beside the cup a piece of pneumonic lung, two inches thick (dense enough to sink in water), and while looking at the shadow of the lung on the fluorescent screen, which was held above the cup and the lung, I poured, into the cup, water until its shadow corresponded in darkness to that of the lung. I then measured the depth of the water in the cup, and found it was two and one-quarter inches. Further experiments showed that a much congested lung, one inch thick, which did not sink in water, corresponded in its shadow to one inch of water in the cup; also that a normal lung blown up to about the distention of full inspiration corresponded, where it was *three inches thick*, to only one-quarter of an inch of water. In other words, a lung in pneumonia or tuberculosis will, if sufficiently diseased, offer ten times, or more, as much resistance to the X-rays as the healthy lung —that is, a very dense lung in either pneumonia or tuberculosis offers about as much resistance to the rays as the liver, and a much congested lung offers several times as much as a healthy one. One lung which I examined was normal except for a small red nodule, less than one-half an inch in diameter. When this lung was looked at under the fluorescent screen this nodule was seen as a dark spot on a bright ground, the whole thickness of the lung casting less shadow than the small, dense substance. These experiments show that congestion increases the opacity of the lungs to the rays, and certain amounts of it, or of œdema, even if there were

no other pathological process, may sufficiently increase their density to cast a shadow on the fluorescent screen, and yet perhaps not increase it enough to give marked signs by auscultation and percussion.

Further, the lungs are brighter in full inspiration than during expiration. If a pasteboard box containing water, one-half an inch deep, be held over the chest of a large, healthy man its shadow may be seen in inspiration, but not in expiration.

It is perhaps best to repeat again the statement that the diagnosis of tuberculosis is not made by an X-ray examination alone, but it does give early warning in some cases of a departure from the normal in the lung which puts us on our guard and enables us, in conjunction with the history, etc., to make the diagnosis of tuberculosis, and thus in many early cases to arrest its progress by proper care.

The following case is suggestive :

C. B., a young man, whose father died of tuberculosis, was brought to me for an X-ray examination and for consultation. He had a history of recent rapid loss of flesh, but there were no physical signs, no temperature, no cough, and the pulse was not rapid. Both his physicians suspected tuberculosis, and he was being kept under observation until more definite signs should develop. The X-ray examination showed a darkened apex of one lung and a much shortened excursion of the diaphragm on the same side. My advice was that he should be sent directly where he could have good food and lead an out-of-door life for some months. This advice was followed. Four months later his family reported him better than he had ever been at any time in his life.

I believe that we may regard the successful care of early tuberculosis as dependent largely upon maintaining the condition and well-being of the patient at as high a point as possible.

The X-ray examination is also useful from another point of view. Physical signs of increased density at the apex and râles, if not confirmed by an X-ray examination, may

be questioned in cases where the history and other evidence do not bear out these signs. I have seen seven cases where an X-ray examination prevented my taking too unfavorable a view, though the physical signs indicated beginning tuberculosis. In none of these has tuberculosis developed, and in such of them as I could test with tuberculin there was no reaction.

In doubtful cases two X-ray examinations should always be made at an interval of some days, in order that the second examination may verify or disprove the first, as there are various conditions, that I have referred to elsewhere, which may temporarily give signs similar to those observed in pulmonary tuberculosis.

In studying tuberculosis by the X-rays we find usually that the apex is darkened and the excursion of the diaphragm is shortened;[1] but we may find now and then only a general diminution in the clearness of the outlines of the lungs and of the ribs. This latter condition is found probably in cases where the disease is disseminated, but it is not easy to recognize this slight departure from the normal unless the physician is in constant practice with the fluoroscope.

Acute miliary tuberculosis sometimes offers difficulties in diagnosis, and is confounded with some other disease, typhoid fever, for example. An X-ray examination of the lungs, by showing a marked shadow on the screen, though at the same time these organs are hyper-resonant to percussion, may direct the physician to a correct diagnosis. I examined with the X-rays a lung taken from a patient who had died of miliary tuberculosis, and found that where it was two inches thick it cast a shadow on the screen as dark as that made by water one and one-quarter inches deep in an aluminum cup.

I have seen two patients about again who had, I think, miliary tuberculosis. The fluoroscope showed dark areas in their lungs, and they reacted strongly to tuberculin. They were too weak to feed themselves and feeble beyond any

[1] The shortened excursion of the diaphragm may be due to changes in the lung itself or to pleuretic adhesions which prevent the lung from expanding.

probable hope of recovery, but their health improved, and with it the appearances on the screen as shown by several examinations.

Other uses which deserve brief mention. The X-ray examination enables the physician to recognize some *old* or *latent areas* of tuberculosis which might otherwise be overlooked. In some of my patients I have found well-marked evidence of former disease by means of the X-rays, and on inquiry have learned that the original trouble dated back in some cases twenty or thirty years, and in others over forty years. The recognition of old areas of tuberculosis by the physician enables him to guard his patient against the dangers of lighting up the disease afresh.

Tuberculosis and pleurisy with effusion. I have seen some cases of pleurisy with effusion in which the X-ray examination gave evidence of increased density at the apex of one lung, although this was not then detected by physical signs. The testimony of the fluorescent screen was confirmed by the finding of tubercle bacilli or by the tuberculin test.

Tuberculosis with bronchitis. Here X-ray examinations may be of much service. In several cases of bronchitis I found by an X-ray examination an increased density at the apex of one lung, which had escaped notice by physical signs. In nearly all of these cases tubercle bacilli were found later.

Tuberculosis with emphysema. The increased density in the lung due to tuberculosis may be so obscured by emphysema that it is not recognized by physical signs, but this emphysematous condition offers no obstacle to the X-ray examination; and the presence of tuberculosis in some of the cases of this class that I examined was confirmed by the finding of tubercle bacilli or by the tuberculin test. The helpfulness of the X-ray examination where tuberculosis and emphysema are both present is twofold. It not only enables us to recognize tuberculosis where, by physical signs, it is undetected, but also to make a diagnosis of *emphysema* which had previously not been suspected by physical signs on account of the *tuberculosis* from which the patient was suffering.

To sum up:[1] *Incipient pulmonary tuberculosis.* I have seen thus far twenty-four cases, where there were slight or no physical signs, in which X-ray examinations showed signs of pulmonary tuberculosis. In all of these cases the X-ray examination was confirmed by finding the tubercle bacilli or by the tuberculin test. In twenty-two cases I have made the diagnosis of incipient pulmonary tuberculosis in conjunction with other indications of tuberculosis, such as one or more of the following: Hæmoptysis, loss in flesh, morning cough, night-sweats, evening rise in temperature, slight physical signs. In these cases the diagnosis was not confirmed by finding the tubercle bacilli, and the tuberculin test was not used.

I recall two cases of pulmonary tuberculosis in which I found no constant signs by X-ray examinations, though there were râles, and bacilli had been found.

I have recognized by means of the X-ray examinations *tuberculosis with emphysema* in nine cases, *tuberculosis with bronchitis* in eight cases, and *tuberculosis with pleurisy* in thirteen cases. In most, though in not all, of these thirty cases the diagnosis was confirmed by finding the tubercle bacilli or by the tuberculin test.

The traces of old tubercular lesions which were quiescent were apparent to me in a number of cases by means of the X-ray examinations.

I have been assisted by the X-rays to a correct diagnosis in three cases of acute miliary tuberculosis.

In the majority of the remaining cases of tuberculosis in which both a physical and an X-ray examination were made the latter indicated more extensive disease than did auscultation and percussion. Sometimes only one lung was suspected by auscultation and percussion when an X-ray examination showed signs in both lungs.

The reports of the Board of Health of the State of Massachusetts, as pointed out by Dr. Abbott, show that out of

[1] Since these figures were made I have seen additional cases of pulmonary tuberculosis in which X-ray examinations have been of service.

1000 persons dying in this State between twenty and thirty years old 597 die of tuberculosis of the lungs. This fact indicates the importance of careful X-ray examinations of persons between these ages and younger who may have any predisposition to or suspicion of this disease.

I think it will be useful to compare briefly the two methods of making X-ray examinations in diseases of the chest, namely, the one by the fluorescent screen and the other by the X-ray photographs, and the description of a case is a simple way to present the subject.

C. P., aged six years, entered the Boston City Hospital, March 16, 1899, with croupous pneumonia. Illness began on March 13th. Physical signs at entrance were as follows: Resonance and respiration good everywhere, except over a small patch in the left back, from the mid-scapula to the lower angle, where there were bronchial breathing and bronchophony.

March 17th, X-ray examination by the fluorescent screen showed a darkened area, two and three-quarter inches wide, extending outward and upward from the left border of the heart. The excursion of the diaphragm on the left side was one-quarter of an inch; on the right side one inch.

31*st*. There were no physical signs and there had been none for four days. *A second X-ray examination* showed much improvement, but the dark area was still readily perceived, though it was not so dark as on March 17th. The excursion of the diaphragm on the left side was one inch, on the right side one and seven-eighths inches.

As on March 31st and the four days previous, there were no physical signs in the lungs of this patient, although the signs of departure from the normal were very patent on the fluorescent screen, the case seemed to me a favorable one in which to make a comparison between the signs of consolidation as recognized on the one hand by the screen, and on the other by X-ray photographs.

Three negatives were made (to see if variation in the time of exposure, or if tubes with different vacuums would bring

out anything), one on March 30th and two on March 31st. On March 30th the exposure was four minutes; on March 31st, three minutes, and a second negative, with an exposure of two minutes, was taken with a different tube on this same day (the tube was in all cases three feet away). From these negatives I had prints made, and *in none* of them did I find evidence of increased density in the left lung as compared with the right.

CASE II.—E. L., a young woman, aged eighteen years, entered the hospital, February 28th. She had pneumonia involving the lower lobe on the left side, which began about one week before entrance, February 21st. There was nothing unusual about the progress of the disease. Four X-ray examinations were made, March 1st, 11th, 23d, and 30th, respectively. Neither on March 20th nor afterward were there physical signs by auscultation and percussion; resonance and respiration were good throughout; but I found signs of consolidation by the fluorescent screen, as indicated by a darkened area and shortened excursion of the diaphragm when compared with the right side. An X-ray photograph taken at this time did not indicate disease, and there were no physical signs.

To sum up: On March 30th in neither of these patients were there physical signs, nor did X-ray photographs taken on this day give any indication of an abnormal condition of the lung, but well-marked signs were seen on the fluorescent screen.

Again, in two cases of incipient tuberculosis in young women in whom the disease was recognized first by an X-ray examination with the fluorescent screen I could see no difference in the two sides by the X-ray photograph. In one of these patients I found no physical signs at the time the X-ray photographs were taken; in the other, where the X-ray photograph was taken some time after the disease had been recognized on the screen and the disease had progressed, there was slight increase in tactile fremitus and a prolonged expiration on the right side.

The cases above mentioned lead me to believe that in these diseases we have in the fluorescent screen a more ready, convenient, and delicate method of recognizing an abnormal condition of the lung than we have in X-ray photographs, and the screen has the advantage of revealing the mobility of the diaphragm and heart.

If simple precautions are taken there is not the slighest *risk* even of an X-ray burn. I have examined about 3000 patients without a single case of injury or of inconvenience even.

I wish to emphasize the fact that signs may be obtained by an X-ray examination when the disease is limited not only to one side, but is just beginning even on that side, before there are physical signs in the first lung or cough.

It is interesting to note that X-ray examinations show that the disease begins oftener in the right than in the left apex.

In conclusion, I desire to urge the adoption of a method which aids in the early detection of pulmonary tuberculosis. First, because an early diagnosis gives the best opportunity to arrest the disease; and, second, because patients may then be taught early simple precautions which will prevent them from being a source of contagion to others should cough and expectoration develop.

I desire to record again my appreciation of the value of Dr. William H. Rollins' investigations, published in the *Electrical Review*, which have promoted the usefulness of the Röntgen light in medicine and in surgery, and to express my gratitude to him for his untiring readiness to aid me to overcome the obstacles which have beset those who are using X-ray apparatus.

DISCUSSION.

Dr. Knight: I do not think there is anyone here who can really say much from practical experience in the discussion of this paper. As I said last summer, there can no longer be any doubt about the value of this method of diagnosis. Dr. Williams is adding to our knowledge of the subject. I have had the opportunity of examining quite a number of his cases, and in many of them, although I could find no physical signs by auscultation or percussion, the rays revealed signs which may indicate incipient tuberculosis. The fact should be borne in mind that physical signs of consolidation should not be looked for at a very early stage of the disease. I see patients for months in whom I can find distinct localized catarrhal signs without any signs of consolidation or any change in the percussion note. I hope that men who have had practical experience with the Röntgen rays in this connection will continue the discussion.

Dr. Stubbert: I was interested in listening to Dr. Williams' paper, and comparing his remarks with those made last year. This comparison shows conclusively that satisfactory progress is being made from month to month in this field of research, and it is apparent that nothing in the treatment or diagnosis of pulmonary tuberculosis has made such great strides recently as the use of the X-rays, especially in diagnosing incipient cases. Another point of interest is the relative value of the fluoroscope and the fluorescent screen. Last year I stated that I had abandoned almost entirely the use of the former for the latter; since then I have not used the fluoroscope in more than three or four cases, as the screen gives a much better picture.

Dr. Williams' remarks have strengthened me in my belief that radiographs are not satisfactory. I have never been able to successfully photograph an incipient infiltration in the lung, and others have had the same experience.

I am fully in accord with the statement which has been made that the use of the X-rays should be invoked in every doubtful case of pulmonary tuberculosis. As Dr. Otis has said, we cannot depend absolutely upon tuberculin. The peculiarities of the patient may mask the diagnosis. I have yet to see an incipient case that could not be accurately diagnosed by means of X-rays. We may sometimes be misled by a slight thickening of the pleura at the apex, but this occurs very rarely, and such a condition, probably, cannot exist without some slight underlying morbid process of the lung. The X-rays are also valuable in cases where there are scattered foci throughout the lung, which, on account of their smallness, do not yield

physical signs. These cases can be very accurately diagnosed, and the area definitely mapped out by means of the fluorescent screen.

I was also interested in what has been said regarding the use of the X-rays in emphysema and bronchitis. In the former, very definite physical signs have been obtained, but in bronchitis we have not been able to do anything with them.

Dr. Langmaid: I am astonished at the limitation of the movement of the diaphragm with such slight abnormal conditions of the lung. I never suspected it before.

Dr. Williams (closing the discussion): In nearly if not quite all of the cases to which I referred in my paper there were no râles. As regards the value of X-ray photographs in comparison with the fluorescent screen, thus far, in my experience, the latter will detect many abnormal conditions earlier and better than the former. I also regard the fluorescent screen as superior to the fluoroscope.

INTERMEDIATE ALTITUDE FOR THE CONSUMPTIVE INVALID.

By B. P. ANDERSON, M.D.,
COLORADO SPRINGS, COLO.

The object of this paper is simply and briefly to call attention to the writer's experience during the past few years with a class of patients afflicted with pulmonary disease, such as have been recommended to and have tried both extremes of altitude without obtaining appreciable benefit; and also to record as nearly as possible the subsequent histories of these cases.

It is to be taken for granted that the majority of the medical profession, and especially the members of this society, recognize (notwithstanding the indorsement and advocacy of individual members, enthusiastic and otherwise, of the various drug, serum, and other treatments advised for the relief of pulmonary phthisis) that the greater benefit to be expected must be from climate, and that climate alone offers the most salutary and most permanently beneficial results.

We must admit that the different climatic resorts act more or less favorably according to the constitution, temperament, and actual condition of the individual case; also, it must be admitted that in no climate with which we are at present familiar can such perfection be found as to justify the opinion that each and every case will receive the relief that is sought. No one climate can be considered as a "cure-all," the panacea for each and every individual case. No matter how honest our intention, how zealous in arriving at a proper discrimination and selection, the fact confronts us that the

matter of climate is largely one of experiment, and that no man can advance a positive opinion as to the result to be derived. The reason for this is obvious. The history and nature of disease, however similar its manifestations, cannot be successfully treated and combated in any two persons by the same methods or by the same routine remedies. This more particularly holds good as to climate and its effects upon different individuals who may be suffering with the same disease. Take, for instance, two identical cases of pulmonary lesion, identical in every respect as to personal temperament, amount of area of disease, general symptoms, etc., and we find that the one will steadily lose ground; the disease, instead of becoming arrested or retarded, will advance, and often, if ordered to another and essentially different climate, benefit and frequently ultimate recovery will result to the failing patient.

With accumulated experience furnishing evidence that location, as to climate, altitude, etc., is the important factor as to results, manifestly it becomes necessary and of vital importance to the patient for the physician to advise change of location for that class of cases failing to improve or become benefited.

As before stated, we admit advantages possessed by different degrees of altitude. We can only arrive at results, beneficial or otherwise, by individual experiences, and the progress, good or bad, of the individual case. Approximately, high altitude to-day, as for the past several years, is recognized as possessing superior advantages and offering results to the average pulmonary invalid of the greatest permanent benefit. The high altitude climate of Colorado or Colorado Springs, with which I have had experience for the past twenty-eight years, is especially adapted to and curative in the majority of incipient cases of phthisis (of whatever variety or type). Therefore, we point with pardonable pride to the unchallenged fact that the reconstructed or cured invalid has been primarily the means of founding an empire, has brought his environment from that of a barren

plain, or rugged, inaccessible mountain fastness to the civilization of to-day—a densely populated region, with all that goes to make the desirable in culture, refinement, and education. When we consider that the pulmonary invalid has alone brought this to pass we can more fully appreciate the possibilities of climate as a factor in the cure of this disease. There are cases seeking this climate and altitude, however, who do not receive the expected relief. Many invalids whose physical condition, area of disease, and length of time affected would presuppose a favorable diagnosis, yet find the altitude affording no relief, but on the contrary often proving disastrous. Perhaps the result cannot be traced (as many may be) to imprudence and mode of life, overindulgence in exercise, etc. My experience has been that in some of this class of cases benefit is received by a change to an intermediate altitude, and for the past several years I have urged such cases to seek temporarily an altitude of from 3000 to 4000 feet above the sea-level, and, if benefited, to remain until all active symptoms disappear. Following this advice, the rule has been that such cases were enabled to return to the higher altitude of Colorado Springs, and upon the second trial improvement continued. Experience has again taught the pertinent fact that the class of cases coming under observation which do not improve or show evidence of a subsidence of active symptoms after a residence of five or six weeks will not be benefited by longer stay, but, on the contrary, will continue to lose ground, and should be ordered a change at once. In advocating the change to a lower or intermediate climate I would be understood as meaning those cases which we term curable, or at least in which a favorable prognosis has been given. In the selection of the most favorable climate for such cases I was largely aided by having my attention called, some eight years since, to the Mesilla Valley, in New Mexico—a valley situated about forty-five miles northwest of El Paso, Texas. Invalid patients coming to Colorado for the cooler summer months, after having spent the winter in this valley, were the first to attract my atten-

tion. Their enthusiasm as to dryness, sunshine, the greater number of days one could remain in the open air, etc., did not impress me so much, all of which advantages we have here in Colorado, as did the fact of their improvement, which could only be attributed to the advantage of altitude, which averages from 3500 to 3800 feet, combining with its lower altitude the important essential of a maximum amount of sunshine and dry air. Commencing with the fall and winter of 1892, I have regularly advised those patients who for the most part did not progress favorably in Colorado to seek this climate. Since 1892 I have a record of somewhat more than 200 cases who spent the winter months in this climate. My record embraces many cases in the various stages of advancement of disease, and many were complicated with laryngeal tuberculosis. The subsequent history of these cases shows that a large majority, or 70 per cent., improved and are still living. In the first few weeks upon making the change to the lower altitude marked improvement was noted in every case. Active symptoms which persisted in the higher altitude subsided, which was immediately followed by a gain in flesh and strength. The majority of these cases returned to Colorado for the summer months, and a few returned for shorter visits to their homes in the East. Many of these cases, after having spent from one to three winters in this intermediate altitude, find that a return for permanent residence in the higher altitude of Colorado is productive of no ill effects, but, on the contrary, they continue to improve and are permanently benefited by the change. These cases upon re-examination were found to have gained complete arrest, freer expansion, greater elasticity of lung tissue, and normal respiration.

Patients inform me that they are enabled to remain out of doors the majority of days from October to May, and from the hours of 9 A.M. to 5 P.M. The disadvantages are to be found (as obtains in most cases in New Mexico) in the lack of suitable accommodations, diversion, etc.

Briefly, and I am aware not concisely, I have given my

experience with a class of cases the majority of whom have tried lower elevations, such as some of the Southern resorts, California, etc., before coming to Colorado, and failing to receive benefit previous to attempting the intermediate altittude.

The selection of the proper climate for the individual case is appreciated as of the greatest and first importance, and yet how difficult. It has been said long ago that the "individual result depends on the individual." No man can predict with absolute accuracy, in sending a patient from home, just what the result will be. I have known invalids apparently hopeless who were advised to return to their homes with the expectation of certain death in a few months, persist in remaining and living for years in Colorado after such advice had been given. A few years since a physician who was then residing in Colorado Springs, and who had served on Surgeon-General Esmarch's staff during the Franco-Prussian war, walked into my office and informed me that he had been invited by a United States Army officer to accompany him overland to Fort Davis, Texas, a distance of several hundred miles. The man was apparently in the last stages of phthisis. The whole of the left lung was involved, together with the apex and to the third rib of the right. There was a large, suppurating cavity in the upper lobe of the left. The physical condition was one of emaciation and prostration. I frankly told the doctor that he would not live to reach Santa Fè—one-fifth of the distance. He persisted in starting on the journey, and not only reached his destination, Fort Davis, but improved to such an extent as to receive the appointment of acting assistant surgeon, and lived for seven or eight years afterward, and I am informed died ultimately with some affection of the heart. It is my belief that climate and proper environment and professional advice are the most rational, and, I may say, the only remedies. If the individual finds a climate that proves efficacious in his special case, whether it be a high or low altitude, that is the climate in which he should remain as long as necessary to effect recovery.

DISCUSSION.

Dr. Leonard Weber, of New York: I think it is safe to say after listening to this paper, that the medical profession, both in this country and on the other side of the Atlantic, are all agreed that what the consumptive wants is pure air, and I think the time is approaching when high altitude will not be considered of such paramount importance in this connection as it was some years ago. This change of opinion has been brought about by the fact that good results in the treatment of phthisis are being obtained in sanitaria situated at altitudes of only 300 feet, or even less, above the sea level.

It has always been my practice, in the treatment of incipient pulmonary tuberculosis, particularly when the disease is unilateral, to send such patients—providing there be no contraindications in the way of heart or kidney trouble—either to Davos, in Switzerland, or to Southern Texas or Colorado, because I have found that in those regions they were more apt than elsewhere to recover from their disease, or at least have its progress stopped.

In a second class of patients, where the disease is more advanced in one lung, and I suspect beginning infiltration in the other, together with considerable fever and an irritating cough, my experience has taught me that they do better at modern altitudes, say about 3000 feet above the sea level, in regions such as the author of the paper mentioned. Of course, we all know of even advanced cases, with hæmoptysis, that improved at very high altitudes, but I know of other instances where such patients, when sent to Colorado, did not do well at all, and had to return to much lower levels.

Dr. Solly: I think all of us who practice in Colorado recognize the value of lower altitudes, such as are found in New Mexico and Arizona, in certain cases of pulmonary tuberculosis, particularly in those where there is a continuously rapid pulse and high temperature, and in advanced cases, such as Dr. Weber referred to. They are the patients who are unable to take much exercise, and who are subject to catarrhal attacks when exposed to changes of weather. The younger do better in high climates; cold seems to agree with them much better than warm air and low altitudes.

One thing we have to contend with in the lower altitudes of New Mexico and Arizona is the lack of proper food in those regions. There should be better facilities in that section of the country for feeding and taking care of the patients who are sent there. Tucson and Oracle would be very good places for these patients if they could receive proper food and care there. The same applies to various

towns along the line of the Rocky Mountains which are situated at various altitudes.

Dr. Newton: In connection with this subject of sending our patients away to various places, where they may get the benefit of the altitude, we should not lose sight of the question of proper food to suit an impaired or feeble digestion. I have seen men out on the frontier who could eat almost anything and thrive on it; but when we are dealing with patients who suffer from the indigestion so common in pulmonary tuberculosis they had better be kept at home, unless we are convinced that they can obtain proper food elsewhere, because we cannot fight tuberculosis with poor food. The digestion should always be kept well in sight—much more so than it usually is. A cheerful disposition and good digestion will help a man to recover from pulmonary tuberculosis at home or elsewhere; while a man who is easily discouraged, and who suffers from poor digestion, will be handicapped in the best of climates.

WHY FUMIGATION OF APARTMENTS OCCUPIED BY TUBERCULOUS PATIENTS AT HEALTH RESORTS SHOULD BE UNDER MUNICIPAL CONTROL.

By CHARLES F. McGAHAN, M.D.,
AIKEN, S. C.

In bringing this subject to your notice I fear some of you will think that I am speaking on a theme that has been fully discussed before, action having been taken upon it by the Boards of Health of several large cities, as New York and Philadelphia; but it is just for this reason that I want the members of this Society to realize the dangers to which their patients are exposed when they send them to the so-called health resorts of this country. I claim it is imperative that every health resort where we send our patients should compel the fumigation of apartments occupied by tuberculous patients to be done under the auspices of the sanitary authorities.

I have seen apartments in which have been the worst cases of phthisis disinfected in the most superficial manner, and I have almost shuddered a few days later, when called to these same rooms, to see a case which was showing only the first symptoms of phthisis. Knowing these rooms must be reeking with the tubercle bacilli, I would insist upon the immediate removal of my patient to more hygienic quarters; but there are many cases that take apartments, without consulting the physician, for some weeks or months, and by that time they would have given the germs every opportunity to contaminate the system. We have some landlords and boarding-

house keepers who attempt the disinfection of a room, but, from ignorance of the methods of doing it properly, simply make a farce of it, and at the same time announce that it is perfectly healthy, as everything has been thoroughly fumigated. I remember once having my attention called to a room that was being fumigated; the landlord had carefully pasted paper over all the cracks and keyhole, but had forgotten to close the transom.

Now, if we are so careful at home to have our patients avoid all source of contamination and self-inoculation we should be equally as cautious in selecting a location for them in which to spend the summer and winter, and prevent them from visiting places where the rooms of the hotels and boarding-houses are teeming with tubercle bacilli. Sanitation in most of the small towns of the interior is carried out in its crudest form, and since the contagiousness of tuberculosis has been proven, and the opinion of the profession in regard to it is so well known by most of the laity, we find the larger and better class of hotels at the different resorts refusing to take tuberculous patients; hence it forces that class of patients into apartments owned by people who either do not believe in the contagiousness of the disease or are simply trying to obtain what remuneration they can for their rooms, and in both cases the disinfection is insufficient. Every room offered for rent at a resort frequented by tuberculous patients should have a certificate attached to it stating it to have been thoroughly disinfected and inspected under the auspices of the Board of Health or sanitary inspector, and that it is, in the opinion of the said board, perfectly healthy. Then we should advise that the local health authorities should pass suitable ordinances by which they can control the nuisance of expectoration on the streets and in the parks; also to regulate the laundrying of clothes of tuberculous patients, which should be washed apart from those of healthy people. I consider it our duty as a society to exert our influence to compel the authorities at the different resorts to pass the proper ordinances to protect the patients we send to those places from

the avoidable dangers of contagion. We should insist upon their preventing expectoration on the ground, and we should force them to prohibit the washing of handkerchiefs and cloths upon which the patients have expectorated, and in their place substitute handkerchiefs of cheese-cloth, to be immediately burnt after use.

In my small sanitarium in Aiken I have enforced the strictest sanitary measures, and am pleased to say that I have so instructed the patients in regard to the laws of hygiene that they consider it necessary to conduct themselves as educators to the townspeople. We are now introducing a system of sewerage in Aiken which is considered to be the best known in the world for an inland town, and is the one so successfully experimented with by the Massachusetts State Board of Health and adopted by them in preference to all others for their interior towns.

The perfecting of the sanitation of Aiken will make the town as conducive to the promotion of health as any resort of the kind I know of, especially since this last improvement is but another advantage added to the superior features of climate and hygiene with which nature has so richly endowed this little town among the pines of the Southland.

In conclusion, I beg to add the hope that I have the hearty co-operation of this Society in furthering the adoption of the idea herein mentioned.

DISCUSSION.

Dr. E. O. Otis: This paper is on a very practical subject, and I hope the reader of it will outline a little more definitely the plan of disinfection or renovation he would suggest at Aiken or any health resort. In Boston the Board of Health is willing to disinfect any apartments which have been occupied by a consumptive, but this is not compulsory. The best system I know of is that carried out in Cannes, in France, where a check system is in vogue whereby the proprietor of a hotel or villa is notified by the attending physician that a room or apartment which has been occupied by a tuberculous patient, or in which one had died, must be disinfected

or renovated before it can again be occupied. The proprietor takes this notification to the sanitary authorities; the disinfection is made by them, and they return the original card of notification to the physician, with a note made on it that the disinfection has been made. The physician then is assured that the room or apartment is ready for occupancy again. In addition, there is kept at the Mayor's office in Cannes a list of the hotels and villas whose proprietors agree to conform to all the suggestions made by the physician as to disinfection and renovation. Whenever a physician is asked to recommend a hotel or apartment he has this list, which is public, to refer to. This arrangement is both for the interest of the proprietors and the safety of the visitors.

Dr. Charles F. Gardiner: Although I am fully in accord with those who would adopt stringent measures looking to the proper disinfection of apartments which have been occupied by tuberculous patients, I believe that the danger that may result from such apartments which have not been fumigated has been somewhat exaggerated. I do not believe that the average case of pulmonary tuberculosis results from the inhalation of the tubercle bacilli. Recent experiments made in Germany go to show that a large proportion of persons are tuberculous in any case, and have been for years without physical signs, and to say that such individuals, when their latent disease becomes active, have recently contracted consumption, is a mistake in most instances, possibly the germ was taken in milk in infancy. As to the dangers of contagion in health resorts, I have made quite a number of experiments in Colorado Springs. I took dust from walls of rooms occupied by consumptives, *e. g.*, in the Antlers Hotel, and in no case did guinea-pigs inoculated show tuberculosis. While I certainly encourage strict cleanliness and disinfection on general principles, I at the same time consider that more attention paid to good food, and, above all, open windows and light, will do more to limit extension of tuberculosis than anything else.

Dr. Guy Hinsdale: I appreciate very highly the importance of the suggestion contained in Dr. McGahan's paper, and I believe they would do much good if they could be brought to the attention of hotelkeepers in a manner that would appeal to the success of their establishments. The Pennsylvania Society for the Prevention of Tuberculosis has issued a tract on this subject which has been widely circulated, over 10,000 copies having been sent out. It is entitled "How Hotelkeepers can Assist in Preventing the Spread of Tuberculosis." We ought to decide upon a practical and efficient method of disinfecting an apartment which has been occupied by a tuberculous subject. Probably the best way is by the action of sunlight. In addition to this, or instead of it, some advise scrubbing with a strong corrosive sublimate solution or the use of formaldehyde. A

paper drawn up by this Association and widely circulated would do good.

Dr. McGahan: This subject has interested me for many years, and it is now being agitated in my home at Aiken, where some members of the Board of Health are in favor of adopting a system of fumigation, while others are not. Unless the fumigation is thorough, it is practically useless. The apartments at the various hotels are successively occupied by different classes of patients, some of whom are careful about expectoration, while others are not. I have in mind now an apartment in one of the hotels at Aiken which was occupied last year by a nephew of the late Dr. Pepper; this year it was occupied by a Canadian-French family who were very filthy. I do not think that we can rely upon the proprietors themselves to attend to the proper disinfection of their apartments; a regular system should be organized and carried out, and, it seems to me, that the health authorities are the proper persons to take hold of it.

SOME REMARKS ON CLIMATE AND RENAL DISEASES.

By JAMES B. WALKER, M.D.,
PHILADELPHIA.

At a meeting of this Association held in 1888, Dr. J. C. Wilson read a paper on "Climate and Bright's Disease," presenting the views held by the leaders of thought in diseases of the kidneys; and more recently Dr. Solly, in his admirable work on *Medical Climatology*, has given a comprehensive epitome of the same as viewed by himself and the various authorities of the day.

There remains, perhaps, but little to be added as regards the general aspects of the question except to verify, modify, or correct the conclusions already arrived at and to specialize and individualize as observation and experience suggest.

The causal relation of certain climatic conditions for and against affections of the kidneys has been sufficiently established. Their prevalence in temperate climes, where variability of temperature is accompanied with moisture, as is found in the inland portions of the Atlantic water-shed in our own country, compared with the infrequency in torrid regions, is affirmed by all authorities and abundantly supported by statistics; and while all admit that the habits and modes of life of the inhabitants of the former have something —no doubt, much—to do with this state of affairs, yet that the climatic conditions *per se* bear a directly causal relation in precipitating such attacks is thoroughly appreciated.

The condition of climate acting in this causal manner are variability of temperature associated with dampness. The dampness is not alone presented by the amount of moisture

contained in the atmosphere, but, what I believe to be of much more vital importance, the dampness of the soil.

As our immortal Bowditch pointed out long ago as of prime importance in choosing a climate for phthisical patients to be a dry, porous soil, and *per contra*, that a damp soil was an important factor in inducing or favoring the development of phthisis, so I have no doubt the climatic factor of prime importance in inducing renal disease is dampness of soil.

Next to the respiratory, no system suffers from improper climatic conditions so seriously as the renal. Tucked away though these organs be, in the warmest and most protected portion of the animal economy, yet, through the workings of the vasomotor apparatus, climatic variations directly impress them.

If, as is frequently the case, and increasingly so with advancing years, they are in a condition of engorgement due to their efforts at elimination of an excess of or an imperfectly metabolized tissue waste, or from other irritating ingredients of the blood, as alcohol, lead, etc.; if, while in this state the skin, insufficiently protected, be subject to cold, and especially damp cold, the transformation into an inflammatory state is easily made.

If this is repeated year in and year out pathological changes must, of necessity, result and more or less crippled kidneys be the outcome.

It is for such crippled kidneys that climate furnishes the most relief. They do their work under favorable conditions well, but quickly show their imperfections if unfavorable conditions arise. Such kidneys secrete an abundant urine of low specific gravity, ranging about 1010, without albumin, usually acid in reaction, and sometimes precipitating the urates on cooling. If the possessor of such a pair of kidneys has an attack of acute disease he is apt to present an albuminous urine, with casts, and life may be in jeopardy, or, as is more frequently the case, renal inadequacy is exaggerated by an increase of the crippling process.

It is with this class of cases that variability of temperature

may accomplish the same deteriorating influence, in some cases inducing an attack of acute Bright's disease; but in a vastly large number stopping short of this, giving an albuminous urine, not smoky or possessed of blood casts, but often associated with evidences of imperfect depuration of the blood. Among these evidences are headache, usually occipital; anorexia, running into nausea and occasionally vomiting, and sometimes slight vertigo.

These symptoms are present on awaking in the morning, and sometimes wake the patient two or three hours before his accustomed time. They may shade away before or immediately after breakfast; are always absent or greatly diminished by 11 A.M.; sometimes to return about 4 P.M., not quite so severe as in the morning, and absent entirely by bed-time in all but the severest cases.

For this class of cases a proper climate will do much to restore the renal equilibrium. Such cases are usually in perfect health during the summer months, when the skin is active and an out-of-door life possible; but with the onset of the cold and dampness of early fall they return each year, with perhaps increasing vigor, unless recognized and carefully protected, and often in spite of the utmost precaution, as the kidney changes become more and more marked.

During the past winter (1898 and 1899) a large number of such cases came under my observation. The extreme variability which marked our Eastern coast climate from early September, through the winter and until the middle of April, with rain or snow-fall at short intervals, the air was constantly saturated with moisture, and at least in the cities the streets continuously wet, the number of respiratory diseases were vastly increased and the number of embarrased kidneys greatly multipiled. In some the kidney embarrassment accompanied the catarrhs of the air passages, modifying their course and retarding the convalescence. In others it existed alone. It was usually easily controlled, and disappeared in a few weeks; in other cases it developed into acute catarrhal nephritis.

So much for the causal relations of climate.

As for the remedial and therapeutic value, I will point, first, to the improvement of this class in the warmer, dryer summer months in the same latitude and locality. The modified diet of the summer, with its diminished meats and increased vegetables and fruits, no doubt plays no unimportant part in this improvement; but where this feature is persisted in after the inclemencies of the winter appear, and systematic exercise, scrupulous attention to the skin and bowels and other hygienic matters have been most carefully persisted in and adhered to, and yet in spite of this the one factor of climatic changes overtops all and reproduces former experience, one is forced to attribute not a little, but the most of the summer's improvement, to the climatic conditions associated therewith.

In those cases where attention to diet, skin, and bowels, systematic exercise, etc., with such medicinal aid as may seem wise, fail to prevent attacks of renal incompetency during the winter season, I have found a sojourn at Lakewood or Atlantic City of vast benefit.

Here the sandy soil dries quickly after a storm; snow soon disappears, and both soil and air are constantly drier except during the actual rain-fall.

The appetite is usually rapidly restored as the nausea disappears, the headache vanishes, sleep is undisturbed, and in a few days the change is almost marvellous. The kidneys have resumed their equilibrium, and all the functions feel the impress.

This experience, coinciding with one's preconceived notions, and repeated in a number of cases, leads me to present this very imperfect paper.

In conclusion, I would state that as dampness of soil is perhaps the most objectionable climatic factor in the induction and aggravation of crippled kidneys, for those residing where this factore prevails, and for crippled kidneys in general, the sunny, sandy seashore resorts, especially where dryness of air and sunshine are associated with a suitable soil, furnish an aid to relief of unquestioned value.

There is no occasion in the most of these cases to send such invalids to far distant points in search of a suitable climate. In the inland cities of our Atlantic water-shed, and even in those cities near the coast where an impermeable pavement aggravates or perpetuates the dampness, the dry, sandy seaside resorts to be found near at hand from New Jersey to Florida, where comfortable living is possible, and where patients can take their out-of-door exercise dry shod, furnish an easy, ready, and hence available aid to embarrassments of the renal function which constitute the vast majority of renal diseases met with, and which would otherwise drag their weary length along until the warmer and drier climate of early summer brings relief, while all the time histological changes are possibly carrying the crippling into the danger limit.

I have no desire to detract from the great advantages of the drier and continuous warmth of the great Southwest for the more marked cases, nor of the Hot Springs of Virginia and at Richfield to bring recalcitrant kidneys back to their normal equipose. For severe cases these may be requisite. My wish is to give my experience as to the value of more accessible places which supply the requisite aid in many cases where the renal changes are still within the possibility of harmonious work under favorable conditions.

DISCUSSION.

Dr. Thomas D. Coleman, of Augusta, Ga.: When I first went South I was struck by the comparative infrequency of renal disease. I can now recall a few cases among the better class of my white patients in Augusta. We do see it, of course, particularly among the negroes, who are not well clothed, and live amid poor hygienic surroundings. I attributed this freedom from renal disease to the equable climate, which at least is not apt to excite the disease, even if it does not exert a beneficial effect upon it. The mortuary tables of Augusta show a very low mortality rate from Bright's disease. I now recall one man who came from New York, and when I saw him he was on the verge of uræmic coma, with suppressed urine and

enormous swelling of the extremities. I prescribed diuretics, and applied hot fomcutations over the kidneys, under the influence of which he rapidly recovered, passing an abundance of urine of normal specific gravity, and he did remarkably during the three weeks he remained there. He returned to his home in the North before the weather had become settled, and died very shortly afterward of kidney trouble.

Dr. Judson A. Daland: I was interested in listening to Dr. Walker's paper on the subject of the effect of climate, together with the state of dryness of the air and soil on renal disease, and was glad to hear food mentioned in connection with the subject. Probably this food question comes in a little stronger than one might infer from the paper. That a distinction should be made between a winter diet and a summer diet is perfectly clear to us all, and perhaps in the very climate to which he referred the dietetic reactions are shown the soonest. Another point is the quantity of food that is required in different climates. In a warm climate the food required would probably be from 15 to 20 per cent. less than in a cold climate, and in addition to that you get the benefit of freer action of the skin, and also the action of water, which is taken in larger quantities in such climates.

Dr. Newton: I wish to add my testimony to the value of such a climate as that of northern Texas in kidney troubles. I do not remember having seen more than one case of Bright's disease during my four years' residence at Fort Elliott, which is located in what is known as the Panhandle district of Texas. There the climate is very dry, and the people are so dry that they drink all they can get, including a great deal of rot-gut whiskey.

The rarity of Bright's disease in Texas is referred to in the writings of Dr. H. C. Wood. The subject is a very important one, and is certainly worthy of further investigation, especially the question whether a person suffering from nephritis had better be sent to Texas or some part of the Rocky Mountain area or not. This problem cannot be answered off-hand. But its importance renders it, in my judgment, a most fitting subject for the study of this Society, and I hope we shall be able to do some satisfactory work in this field later on.

Dr. Phillips: I would like to say a few words in regard to the meteorological aspect of Dr. Walker's paper. The doctor attributes the benefit derived by nephritic patients who are sent to Lakewood and Atlantic City to the dryness of the soil and atmosphere. The soil of Atlantic City may be dry, but the air there is much damper than it is, for example, at Philadelphia. To explain the beneficial effects derived there by patients, we must look to something beside moisture. Atlantic City has a much more equable temperature than Philadelphia, the diurnal variations being much less; the tempera-

ture does not go up so high during the day nor fall so low at night, and for that reason the patient is better able to protect himself with proper clothing. I think the benefit derived by patients suffering from renal disease in certain regions, at least along the Atlantic seacoast, is due more to the equability of temperature than to any effect of moisture. In very dry, hot climates perspiration is free, and the kidneys have much less work to perform, which probably explains the relative infrequency of renal disease in such climates.

Dr. Samuel A. Fisk: The statement was once made to me by a prominent railroad official in Chicago, that in the stockyards there he could always recognize the Western steers by the fact that they had such perfect kidneys. Personally, I rarely meet with a case of so-called Bright's disease in the Rocky Mountain region. I have, of course, seen tuberculosis of the kidney in connection with pulmonary tuberculosis, and in such instances the renal condition usually improves with the general health of the patient. I have in mind at present a young man who has suffered from marked albuminuria since his college days, ten or a dozen years ago, the urine usually showing at least 2 per cent. of albumin (by bulk) with the nitric-acid test. The man looks perfectly healthy, and has never been prevented from going about and doing his daily work. I know of other similar cases, and these have led me to believe that simple albuminuria is not a contraindication for sending a patient out to Colorado; on the contrary, I believe that patients with Bright's disease do pretty well in that climate.

Dr. Henry Sewall, of Denver: The problem of the relation of climate to renal disease can only be solved through abundant and carefully sifted statistics. There is reason to think that the mean climatic character of a place is much less important than the rate of variation from the mean. Sudden and extensive changes in temperature, humidity, and other meteorological factors are probably most effective in causing irritation of the kidneys. Still, it seems to me an important suggestion that it is probably not the physical changes of climate which form the criterion of the adaptability of a locality to Bright's disease, but the physiological changes dependent thereon. The curves representing the physical and physiological variations do not run parallel. To illustrate: the mean 3 P.M. July temperature of Denver by the dry bulb thermometer is 83°, the same as for Wilmington, N. C.; but the mean wet bulb temperature for the same time is 61.4°, the same as for Winnipeg and Montreal. The former may be called the physical and the latter the physiological or sensible temperature, and the difference is due to the small amount of humidity in the mountain air. In the same dry air the sensible temperature of winter corresponds with that of places much further south. Therefore, it seems clear that in considering the relations of climate

to a disease our curves should represent physiological as well as physical variations.

Dr. L. D. Bulkley, of New York: I was very much interested in the remarks made by the various speakers on this topic, as they have a bearing on my own case. Thirty years ago, while an interne at the New York Hospital, my urine was found to be loaded with albumin. I left the hospital; I lived on a milk diet, and was told that I could not live very long in this climate. I thereupon decided to go to Brazil, and already had my trunks packed when something turned me from my purpose, and I remained at home, and here I am to-day. During these thirty years I have had several severe attacks of albuminuria, and it might be of interest to my Western friends to inform them that one of my sharpest attacks occurred while I was travelling in the Yellowstone Park, where the air is certainly very dry.

While I do not entirely accept the statements made regarding the value of climate in renal disease, I would say that the condition of the atmosphere has a decided influence upon the frequency of micturition in my case. On certain days my urine will diminish to a few ounces, while on other days urination will be very free. As regards treatment, I should lay the greatest stress on the diet and the care of the skin, and by the latter I do not refer so much to bathing as to the constant application of woollen garments to the skin, both night and day. This rule I have adhered to for years. In addition to this, I drink a quart of milk daily on an empty stomach. This causes a certain amount of diuresis.

I have given these detals in my own case to show that it is possible for a person with marked albuminuria to live in New York City for at least thirty years, providing he takes proper care of himself.

Dr. Fisk: The speaker has thrown discredit on the Rocky Mountain climate by referring to his experience in Yellowstone Park. I merely wish to say that the climate in those two regions is entirely different.

Dr. N. S. Davis, Jr.: A few days ago I was looking over Dr. Stillman's book, *The Life Insurance Examiner*, searching for statistics, and I happened on figures relating to this subject—the geographical distribution of renal affections. In that book there is a table giving the mortality derived from 5000 fatalities among insured in each of the following groups of States. In the New England States and New York the mortality from renal affections was 3.5 per cent.; in Michigan, Wisconsin, Minnesota, and Nebraska, 1.4 per cent.; in New Jersey and Pennsylvania, 2.1 per cent.; in Ohio, Indiana, Illinois, Iowa, and Kansas, 1.9 per cent.; in Delaware, Maryland, the District of Columbia, Virginia, Kentucky, and Missouri, 2.1 per cent.; in the Southern States, 2.4 per cent.; in Washington, Oregon, California, Utah, Dakota, and New Mexico, 2.6 per cent.

It would appear from these statistics that the lowest mortality from renal affections occurred in the northern part of the Mississippi Valley, while the highest was in the New England States and New York. The next highest mortality was in the Middle Atlantic States.

Dr. Newton: I would venture to suggest that life-insurance statistics in Northern Texas would be apt to be misleading, as it is rare out there to find a man who has his life insured, and of the men who are sufficiently intelligent and prudent to carry life insurance, probably a majority are invalids who have developed phthisis or some wasting disease since they have been insured, and have visited the Rocky Mountains or the Western plains in search of health. Naturally the death-rate of this class would be high.

Dr. Walker: When I presented this paper I studiously avoided all mention of the general treatment of Bright's disease. I referred to diet, under which I meant to include food and water and milk. Of course, I agree with Dr. Bulkley, that milk is a very valuable article of diet in the treatment of renal affections.

I would like to dispute the statement made by Dr. Phillips that the air of Atlantic City is damper than in Philadelphia in the winter months. I believe that patients whose kidneys are somewhat crippled are benefited at Atlantic City, because of the dryness of the soil and of the feet in out-of-door exercise, and the equability of the temperature. The air circulating about the feet or ankles was certainly moister in Philadelphia during the past winter than it was at Atlantic City, whatever it might have been in the meteorological observatory.

CLIMATE AS IT AFFECTS THE SKIN AND ITS DISEASES.

By L. DUNCAN BULKLEY, A.M., M.D.,
NEW YORK.

That climatic conditions may be an important factor in health and disease, we all know; that they may affect favorably or unfavorably the action and functions of each and every portion of the economy, must be granted; that they must have some effect upon the skin in health and disease, follows without saying.

It would be impossible within the limits proper to this article to cover the ground at all completely, especially that portion relating to the manifestations of cutaneous diseases belonging to different climates, and we must be content with a very superficial survey of the subject, which is one of the greatest interest.

For a proper appreciation of the possible effects of climate on the skin and its diseases it must be remembered what elements are included under the general word climate. The simple definition that it is "the assemblage of meteorological conditions that usually prevail in a given region of the earth's surface"[1] hardly suffices to convey a proper idea of the influences operative in connection with the skin and its diseases, without further elaboration. Climate, as understood medically, must include not only the atmosphere, but also the soil, the drinking water, and even the mode of life belonging to the locality indicated. The atmosphere is influential in respect to its conditions of temperature, barometric relations,

[1] Foster's Encyclopedic Medical Dictionary.

and quality; of the latter we have to consider its moist or dry condition and the presence of ozone, and also the foreign elements it may contain, such as microscopic organisms, or those pertaining to nature, such as the alkali dust of the plains, the balsamic emanations from the pines, the emanations peculiar to the sea, etc., or those resulting from civilization, such as smoke, dust, effluvia, etc.

The character of the drinking water can also be an element of importance in connection with the influence of climate on the skin; this relates not only to its hard or soft nature, but also to the mineral ingredients found in medicinal springs used for drinking and bathing, to which this Association has already devoted some consideration.

Nor must the influences of the mode of life and character of food incident to change of climate be overlooked in estimating the effect produced on the skin and its diseases, for all who have considered the matter must recognize that these play a not unimportant part in connection with the effects produced by change of habitation in regard to diseases of many of the organs of the body.

We see, therefore, that the subject is a very broad one, and well worthy of the consideration and study of those who seek to understand climatic effects on the human race.

We will briefly consider: 1. The effect of climate on the skin in health. 2. The effect of climate in producing disease of the skin. 3. The effect of climate on diseased skin.

1. *The effect of climate on the skin in health.* That climate has an effect upon the healthy skin is evident. The darker color of those inhabiting tropical regions, whether aborigines or of late introduction, indicates that influences are at work which modify the structure of this great organ. The tendency to growth of beard in colder climates, and its relative absence in races occupying warmer zones, also shows an effect thus produced on the nutritive elements composing the skin. The varying conditions of free perspiration—often of a very oily nature—belonging to warmer climates, together with the dryness and more or less shrunken state produced by cold, are

evidences of the effects of climate on the physiological functions of the healthy skin.

Some of the results obtained from a change of climate in various conditions of the system must undoubtedly be attributed to effects produced through the medium of the skin. All are familiar with the fact that no inconsiderable part of the treatment at mineral springs consists in the use of the baths, while in hydrotherapy, as practised in many places, even with plain hot or cold water, the end is attained by effects produced upon and through this great organ.

In considering the effect of climate on the skin it is also necessary to take into account the action of heat and cold on the skin, in the production of disease in other organs. While it is fully recognized that many agencies, including microorganisms, share in their production, it must also be acknowledged that that mysterious process commonly known as "taking cold" plays still a very important part in human sickness. Exactly how it occurs has never been clearly shown, but in some manner a chilling of the surface of the skin does result in a variety of morbid processes affecting various parts of the body, and even the skin itself; whether this takes place through the influence of the cold on the terminal nerves, and so by reflex action, or through disturbance of capillary circulation, or from a checking of the action of the perspiratory glands, especially the insensible perspiration, has never been satisfactorily demonstrated. The fact only remains that climate, including the influences of the seasons, sudden changes, and unusual climatic conditions, has great influence on the system, largely through the medium of the skin, and presumably upon the skin itself in health.

2. *The effect of climate in producing disease of the skin.* That climate has an influence in the production of disease in the skin cannot be questioned, even as we have diseases of other organs peculiar to certain regions of the earth; the most startling illustrations of the general proposition are familiar to all—for instance, the fevers and dysentery of tropical regions, and such diseases as tubercnlosis, goitre, etc., in

other climes. Diseases of the skin vary very greatly in different sections, and there are many which are almost peculiar to certain regions of the earth.

It would lead beyond the province of this Association to enter as fully into this interesting subject as one might be tempted to do, and I will but briefly call attention to some of the principle points.

The most striking of these diseases is undoubtedly elephantiasis Græeorum, or true leprosy, which seems to require certain climatic and racial conditions for its successful development. These are by no means yet clearly understood, for while it flourishes principally in the warmer countries, it has also developed strongly in cold Norway and likewise in Iceland. It is not confined to the seacoast, but it may be found in all its forms also in the interior and on the plains. It seems, however, to require certain climatic conditions for its development; for, while it has spread rapidly after its first introduction into certain countries, as in Hawaii in 1859, it has never increased in the United States, although a considerable number of lepers have come to Minnesota; and while numbers of cases have been seen in New York City, dating back many years, there has never been an authentic case traceable to contagion from them.

Elephantiasis Arabum and other diseases, such as lymph-serotum, guinea-worm disease, etc., due to filiariasis, seem also to require proper climatic influences for their development, for they are excessively prevalent in certain regions and quite absent in others.

There are also many diseases of the skin, which are known to us by name and by description and pictorial representation, which are practically never seen in this country, such as yaws, verruga peruana, Oriental sores, such as Delhi boil, Biskra bouton and Aleppo evil, Aden ulcer, pinta, piedra, goundou, ainhum, and many others unnecessary to mention.

Certain climates also tend greatly to the development of the vegetable parasitic diseases, which flourish most luxuriantly and rebelliously in the warmer regions. Thus the myce-

toma, or fungous foot of India, is almost unknown elsewhere, and tinea imbricata is so prevalent in certain islands of the South Pacific that it affects a large proportion of the inhabitants.[1] The other forms of ringworm also develop profusely in warm climates, and are known by the special names of Indian, Burmese, and Tokelau ringworm, Dhobie itch, etc. Pityriasis versicolor also luxuriates in warm climates, and in one place in India over 4 per cent. of the prisoners in jails were found to be affected.

Animal parasitic diseases also abound in warmer climates, and are often a source of great trouble both to natives and foreigners, illustrations of which need not detain us here.

A study of certain other diseases of the skin as observed in various countries shows great variations in frequency, which must in a measure be attributed to climatic influences.

Thus, pellagra seems to be a disease almost peculiar to Italy; lupus is far more prevalent in Austria; favus in Russia and certain sections of Central Europe; true prurigo, particularly observed in Austria, and so on.

The climatology of syphilis would be an interesting study, but, though accurate data are not very abundant, it would lead us far beyond the proper limits of this paper even to record what is known. The specific poison is undoubtedly the same in all countries, but the manifestations of it vary not a little in different climes, as it is now pretty widely disseminated over the earth's surface. For a long time the disease was thought to be a different one in different countries, so much did the various descriptions given vary from each other; but later investigations have shown that the same disease appears under somewhat different forms in different climates and nations. Thus what was long known as sibbens in Scotland, and which was said by Bell to have affected three-fourths of the population, is now recognized as syphilis, which was then largely communicated to family life, as by eating and drinking utensils. The same is true of the radesyge

[1] Manson: Tropical Diseases, New York, 1898.

in Norway, falcadina in the Tyrol, scherlievo in Northern Italy, frenga in Servia, spirocolon in Greece, and so on. On reading the description of the disease as observed in colder climates one is struck with the emphasis which is placed upon the lesions as occurring upon the mucous membranes and bones; whereas those given of the various forms of syphilis in the warmer climates all dwell much more on the cutaneous manifestations of the disease, which in the warmest climates commonly takes the form of deep ulcerations, analogous to those observed from other causes in the tropical regions; so that it is not at all difficult to see that climatic conditions, at times, may influence greatly the development of this, terrible disease.

It is also not a little interesting to note that while syphilis spreads very easily, and often with very great rapidity, in the warmer climates, as in the instance of Hawaii, where it was introduced in 1778, and also flourishes in temperate zones, it does not thrive in extremely cold climates; thus in Iceland[1] it has been repeatedly introduced, but has very quickly died out, and also in Greenland it has never been able to establish itself.

A study of the statistics of diseases of the skin, as reported from various countries, shows very considerable differences in the relative frequency of various diseases, but it would lead us too far to attempt any full analysis of such peculiarities. Indeed, many of them are due to diet and modes of life, and cannot be wholly attributed to climatic influences, although close study would undoubtedly discover many interesting facts in this direction. Even with the careful attention to diagnosis given by most of our observers in this country, eczema appears in statistics with such frequency as to lead to the conclusion that it is relatively far more frequent here than in warmer sections, which must be in a measure attributable to our raw and changeable climate.

With climatic conditions should also be considered the

[1] Bulkley: Syphilis in the Innocent. New York, 1894, p. 5.

changes in the seasons as productive of disease of the skin, and these, together with the relations in the barometric pressure and humidity, would form a most interesting and instructive line of study; but, unfortunately, we possess as yet very few data from which any observations can be drawn. All are familiar with the effect of high temperature in the production of what is known as "prickly heat," which may begin as a disorder of the sweat-glands, known as miliaria, and then develop into eczema. Boils are also prone to be more frequent in warm weather, probably due to the favoring effect of heat on the pus cocci. Urticaria is more common during changing seasons of the year, and erythema multiforme in the spring and autumn. Eczema, psoriasis, and ichthyosis are commonly aggravated in cold weather, and pruritus hiemalis is peculiar to this season.

In regard to the barometric and hydroscopic conditions of the atmosphere in their actual relations to diseases of the skin, very little is known, but enough is casually observed to indicate that they are often of very great importance. Thus, on a number of occasions eczema patients have predicted the occurrence of a snow storm, even twenty-four hours before its arrival. Many patients with congestive diseases of the skin are observed to be much worse under varied conditions of weather. This is readily understood when we consider the feelings of those in relatively good health when atmospheric conditions interfere with the free and proper action of the skin.

3. *The effect of climate on diseased skin.* Having seen that climate has appreciable effects on the healthy skin, and that it may contribute in inducing disease of the skin, it is quite understandable that it may be an element worthy of consideration as to its effect upon skin already diseased. While the truth of this is apparent, it is a little difficult to illustrate it as satisfactorily as desired, owing to the absence of recorded observations, although everyone engaged in dermatological practice has undoubtedly met with many illustrations of the fact.

To begin with that most common disease, eczema, it has been already mentioned that it is commonly aggravated in cold weather. Damp, raw seasons will also increase or develop anew the eruption, while a clear, fresh, dry atmosphere will invariably be of benefit. It is a repeated observation that these patients will be freed from their eruption in certain high altitudes inland, and be again attacked on returning to the damp seashore. The air along the Great Lakes seems also to predispose to the disease. Eczema patients will also often recover from their disease while sojourning in the South.

Psoriasis will also quite disappear on removal to a warm climate, and from reports it would seem that the eruption is relatively seldom seen in the tropics.

Acne is constantly observed to be aggravated by residence on the seashore and to improve on removal to a high, dry locality.

The vegetable parasitic diseases are infinitely more common in the cities, and abound in New York, owing, of course, to the herding of the children in the schools and at play. But, on the other hand, while these diseases seem to be very contagious in this damp atmosphere, they seem to be relatively innoxious in the rural districts, even where the children also come together in schools. Cases occur there now and again; but the disease seems soon to yield and not to spread, even when little care is exercised. It does seem as if the damp sea air favored the growth of the parasite more than the dry, pure air of the country.

These illustrations could be multiplied, but enough has been said to show that climatic conditions may be of importance in reference to diseased skin, and in certain cases should be taken in account in connection with their treatment.

In defining the several elements to be considered in connection with climate in its broadest sense, which really means the place of residence, mention was made of the waters of the region as agents whose operation might be of importance. This is often an exceedingly important matter, which can hardly be touched upon, partly for want of definite data,

although illustrations in practice are not very infrequent. Both in drinking and in the form of bath the character of the water may be an important influence for good or bad; this refers not only to waters which are recognized as medicinal, but also to such as are used in the common acts of life. Many a diseased skin is made worse by hard or alkaline water, and, on the other hand, some soft waters are found to be peculiarly grateful to the skin, both in health and disease.

There has been a great deal of nonsense or want of sense and judgment, both in and out of the profession, in regard to mineral waters and skin diseases. Various springs have been lauded as good for diseases of the skin on the strength of very slim experience. This is a very large subject, which must be reserved for future consideration. A personal visit to some dozens of the springs best known in this connection, and personal interviews with many hundreds of patients who have tried various springs, have greatly shaken my faith in them as remedial agents, except when used with great knowledge and judgment. It is pitiful to hear and know of the thousands of dollars spent in vain by the numberless sufferers who have in time past wandered about, as sheep without a shepherd, among the springs of the world in search of health. If this Association could aid in collecting reliable data and systemizing and crystallizing our knowledge and experience inregard to the real value of the reputed mineral waters in diseases of the skin, it would confer a lasting benefit on humanity and save untold misery and disappointment.

DISCUSSION.

Dr. Solly: I was delighted to hear this paper, because I was the agent through which Dr. Bulkley was induced to have his name presented as a candidate. I told him that the members of this Association wanted to know something about skin diseases. I think his contribution is extremely valuable, and hope it will be followed by others.

Dr. Bulkley's remarks about the use of mineral waters I think are particularly valuable. I hope we shall hear from him and others regarding this subject in the future.

Dr. A. Jacobi, of New York: As far as mineral waters are concerned, the doctor has promised to give us another paper on the subject some time in the future. We all know that a great many of these waters have been recommended for skin diseases. I believe that those which are thus efficacious exert their influence through their general effect on the organism, and this applies to both the internal use of the waters and the mineral baths. Of the latter, both in this country and abroad, many contain either sulphide of hydrogen or carbonic acid, and this latter I regard as one of the most important ingredients of the waters. It exerts an immediate effect on the peripheral nerves, and in this way has a beneficial effect on both internal morbid conditions and many diseases of the skin which are of the nature of neuroses.

Dr. C. C. Ransom, of New York: I would like to say a few words in reference to the effect of mineral waters on certain skin diseases.

Certain diseases of the skin are undoubtedly much benefited by mineral-water baths, the benefit being derived, as Dr. Jacobi has said, principally through the systemic effect of the baths. The class of skin diseases which are most benefited are those which are dependent upon faulty tissue metabolism, as in gout and rheumatism. This class of diseases is always benefited by the use of mineral waters, used either internally or in the form of baths. The effect is not from any local action on the skin, but from its general action on the system, improving metabolism and tissue-change. Certain other diseases of the skin, such as the milder parasitic diseases—*i. e.*, tinea versicolor, may be directly helped by natural bromine or sulphur water applied to the skin. Diseases like urticaria are benefited by hydro-therapeutic proceedures, by the methods of applying the waters as much as by the mineral ingredients contained in them.

The effect of hydrotherapy on the nervous system should not be overlooked. I think, as Dr. Bulkley has said, that very little is known, especially in this country, regarding the efficacy or non-efficacy of the waters of certain springs in the treatment of certain diseases, and I trust that the subject will be taken up by Dr. Bulkley and others, and that, as the result of investigations and practical experience, we will be able, in the future, to discuss this subject more intelligently.

HYGIENICS OF THE SKIN.

By L. D. JUDD, M.D.,
PHILADELPHIA.

Great strides in the sciences generally have taken place within the last quarter of a century. Medicine and surgery may justly claim to lead the van, inasmuch as through the marvellous energy and skill exerted in these channels human life has been prolonged and human suffering alleviated to a marvellous degree. These fields are so vast that more than has been done, and so well done, could hardly be expected. There are great things yet to be accomplished, and it seems to me that the greatest must be developed in the line of preventive medicine within the domains of hygeia. Drug worship seems to run rampant, and in the mad rush for the elixir of life the medical profession, to a great degree, has ignored the simpler laws of nature. They should be devotees of Hygeia as well as students of her father, Esculapius. To teach people how to live and to avoid disease and suffering is a far nobler, though much more difficult, task than any we have in medicine—to teach how, what, and when to eat and to drink; when to relax, repose, and to sleep; how and when to exercise; how and when to wash and to dress, to secure the proper function of the skin. To teach these and the grand truth that if the proper advice as to these things is given and observed disease and suffering will seldom occur —is, I admit, a gigantic task. If the physician could always deal with people of brains—by that I mean people who will think reasonably and philosophically, and be willing to accept advice from those who have made a study of these things—

then the task would be a pleasurable one. My attention has long been directed to the subject-matter in this paper, which I will make as brief as possible, and it is given as the offspring of experience and practical knowledge.

The proper care of the skin is only secondary to that of the proper treatment of the stomach, and the ignorance and apathy in which people are resting on matters pertaining to the care of these organs, which have so much to do with health and happiness, is truly appalling.

It is the duty of our profession to instruct in these things. By hygienics of the skin I refer to the washing and the covering necessary, which embraces the treatment of the skin in health. It is not necessary to enter into an exposition on the structure and the functions of the skin. Enough to say that it is nature's own covering, arranged to retain and to give off heat and moisture; to breathe in life-giving gases, and to give off noxious vapors and effete substances; that it is a breathing mechanism, and equivalent to a secondary lung. It is so planned as to be a complete protection to the economy in cold as well as in hot weather, but Eve's fall and society's demands in consequence have ordained that it shall be clothed.

That it is the most abused of all the human economy excepting that of the stomach can be readily understood and easily demonstrated. Many there are that think the frequent washing of it harmful; others that deem it essential to purge it by excessive artificial sweatings—such as in the frequent use of the Russian and the Turkish baths, the indiscriminate use of soaps, oils, and fats, harsh rubbings by harsh substances, and, above all, improper material worn as underclothing—not alone improper as to material and quantity, but also as to thickness and weight. Who is there among us that has not run the gamut of all these things in his life, personally and professionally? In the first place, the proper cleansing of the skin is most essential to keep its functions in perfect working order. The first thing in the morning after rising, and, if possible, indulging in from ten to thirty minutes of deep chest breathing and light physical exercises, I

advocate the warm bath of short duration, rubbing the surface of the body with the bare hands and without the use of any soap, followed immediately with the sprinkler, with cold water in force from the spigot until the skin is in a glow. Once—say every seven days—a full bath in hot or warm water, using pure Castile soap and following with the sprinkler with cold water, as on other days. When the full bath or plunge cannot be had, daily sponging with hot and then cold water is the next best. In drying the skin soft linen or crash toweling should be employed. Many claim they cannot endure the shock of the sprinkler with cold water, but after a trial, starting gradually at first if necessary, they will soon feel that a bath is incomplete without its use, and that the skin seemingly craves it. It is a tonic to the heart by its action on the capillary system, as evinced in the rapid reddening of the skin. In this manner of washing perfect cleanliness is obtained, and the skin is kept in proper working order, and does not lose its elasticity. A wonderful sense of vigor is imparted, and when properly clothed the man goes to his daily duties clad in the right spirit—provided always that he abstains from overloading his stomach.

Now, what is the proper covering for the skin? There is now going on in the periodical press quite a conflict over "the underwear problem"—what and how much to wear, when to change; these are the questions sanitary science is asking. It is to be expected that the answers are as diverse as is the individuality of those answering. More are given through theory than from any practical knowledge born of proper reasoning through experience.

The ideal covering for the skin (that which is generally known as underwear), is linen—not finely woven linen, but loosely woven, as that of a mesh. We would not think of attempting to dry a wet face or body with a woollen fabric; it is too slow in absorbing moisture and equally slow in drying. We employ linen, and any close observer knows that the more loosely woven it is the quicker it dries the skin,

and is much more perfect in this respect than is that of any other fabric known; and we also know that when suspended in the air it dries more quickly. Under normal conditions our body loses through the skin from two to three pints of moisture, in the form of evaporation, in twenty-four hours. That material which rapidly takes up moisture, dries the skin and itself rapidly, is the proper covering for comfort and protection. Our skin should be kept dry as well as the air and clothing surrounding it.

"The protective feature of clothing depends upon its air-holding capacity or porosity." "The defect of wool in the power of quickly absorbing and eliminating moisture is a serious one, and if properly understood would do away with its use for underclothing. A woollen undergarment, when first put on the dry body, will impart to the same a feeling of warmth and comfort, which will continue as long as the evaporation of the skin is not in excess of the ability of wool to absorb and eliminate the moisture. However, if in consequence of impaired radiation of heat, as in summer time, or a greater production of heat as by physical exercise, evaporation should be increased, wool will be no longer able to absorb the moisture as fast as excreted from the skin, nor will it be able to part with all it may absorb; hence the skin and the air surrounding, as well as the garment itself, will be moist, and a further evaporation will be greatly interfered with.

"Moisture and velocity of air augment the conduction of heat. If the wet body should under the above conditions be exposed to a draught, a rapid abstraction of heat would at once take place which would chill the body and which usually results in a cold. It is a matter of daily observation that all those who wear wool next to the skin are very prone to contract colds."

"A porous linen, having the advantage over all other fabrics of absorbing moisture and eliminating it quickly, will provide for a dry climate around our body; hence will enable us to stand extremes of heat and extremes of cold with comparative comfort. Having further the property of cleanli-

ness and being non-irritating to the skin, it would seem peculiar that its advantage for the purpose of underclothing should not have been recognized until recently."

The fibres of wool as seen under the microscope are barbed and irregular, while those of linen are smooth and round. The advantages of linen over wool in eczemas and all skin diseases prove its protective and non-irritating qualities. J. C. Milton, senior surgeon to Dr. Johns Hospital, London, for the Diseases of the Skin, says in his paper on the "Hygienic Excellence of Linen:" "For years I have been fighting the battle of linen against every kind of prejudice and opposition. Long before then I had begun to be very skeptical as to the power of woollen material to protect the wearer against the agencies which bring on catarrh, pleurisy, congestion of the lungs, bronchitis, and rheumatism, principally from observing that precisely the persons who most persisted in oppressing their frames with this kind of clothing suffered most from these ailments. I had to learn that faith in woollen is a piece of fetish worship; that while flannel is not in any way more protective than linen, if, indeed, equal to the latter, the use of it constantly brings on illness and miseries from which those who wear linen enjoy complete immunity." I would not have it understood that I am opposed to wool for clothing. It is the proper material for outer garments and top wear, as it is more impervous to the moisture of the atmosphere, and as an outside garment protects from the radical changes of this trying climate.

The days of the woollen fad for underwear, which has been rampant for over a generation, is rapidly passing, and with it will go much suffering from la grippe, pneumonia, pleurisy, and congestion of the internal organs generally. Many of us can remember the homespun linen we wore in our youth, for in the early days it was the prevalining underwear, and the colds and the la grippe which seem to be products of modern civilization were almost unknown.

In the literature of all times in the past may be found evidences of linen as the skin covering. The art of weaving

fine as well as porous linen was well known in Egypt, India, and Greece. Moses, the greatest of all ancient law-givers and physicians, evidently recognized the hygienic properties of linen as underwear, for in Ezekiel 44 : 17, 18, we read : "They shall be clothed with linen garments ; and no wool shall come upon them . . . they shall not gird themselves with anything that causeth sweat."

For fully three years I have thoroughly demonstrated through my own personal experience, in my family and throughout my practice, the priceless value of what I have in this paper advocated. I trust it may be the means of directing the attention of this Society to the importance of the subject, as one well within the scope of our organization.

DISCUSSION.

Dr. Bulkley : Dr. Judd has certainly brought out this subject very well. His views, however, as based upon his experience, are so diametrically opposed to mine that I hardly know what to say. For many years, both summer and winter, I have been wearing underclothing of Scotch wool next to the skin, and I have certainly derived more comfort and benefit from it than from cotton underclothing.

I also differ from the doctor somewhat in regard to the amount of bathing. As a young man I was very enthusiastic about the value and necessity of the daily bath; but after I had been practising medicine for some ten years I read a paper by Hebra, my master in Vienna, in which he discountenanced the too free application of water to the skin, and showing that it was those who bathed most who suffered the most from skin diseases. My own experience since then has taught me that the too free use of water to the skin does harm rather than good. When patients who are suffering from psoriasis, pityriasis rosea, or various other skin troubles ask me whether they can continue their daily bath, I advise against it, and call their attention to the fact that the animals which are most in the water have the most scales. I frequently see patients whose systems are weakened by too frequent bathing. Congestion of internal organs has been ascribed to the too free use of water externally. In private practice I am not in favor of the same amount of bathing that I was years ago. While I grant that the skin is a great emunctory organ, I believe that its function is injured rather than aided by too frequent bathing.

Dr. Babcock: Although I did not wear linen underclothing during the past winter, I think that some of the claims made for it are valid. Two years ago, when I tried the linen-mesh underwear, I found that I could not wear it with comfort in cold weather without another suit of underclothing outside of it. With the two suits of underclothing I found that in my steam-heated office I got into a perspiration, but when I went out into the cold, even with the weather at a zero temperature, I did not feel the sudden change as much as I had been accustomed to. The linen-mesh underclothing, worn alone, did not keep me warm enough in cold weather, but when I wore it next to the skin, and outside of it wore a second suit of light woollen underclothing, I did not appreciate sudden changes of temperature as markedly as I did without the linen-mesh underwear.

Dr. C. C. Ransom: Dr. Bulkley has perhaps overlooked the fact that the majority of the patients he sees are suffering from skin diseases. I do not think there is any real connection between frequent bathing and the production of skin diseases. The Turkish-bath attendant is often in the bath for hours, and he usually has a beautiful skin. Weakness from bathing I believe is due to the fact that the bath is unwisely taken. For example, I regard the cold plunge in the morning as a pernicious thing. At that time I believe a tepid bath, or a bath at a temperature which is agreeable to the patient, barring the hot bath, should be taken. The individual's feelings should be consulted. The plunge, to a person who dislikes it, is injurious. Let him rather stand in a tub which contains five or six inches of hot water, and then sponge the body with water somewhat cooler; this will give him a little reaction and act as a stimulant rather than as a depressant.

Dr. Walker: My experience with the linen-mesh underwear has been very satisfactory. It makes a delightful undergarment, although it is expensive and is not durable. I have patients who have worn this kind of underclothing for one or two winters, and they are very well satisfied with it, taking fewer colds and suffering less from gouty pains. I think the linen-mesh next to the skin, with wool outside, is, for the coldest weather, the ideal underclothing. Some persons perhaps cannot wear it. Skins differ almost as much as mucous membranes do, and we can lay down no inflexible law for skin covering any more than we can for diet.

Dr. Solly: Perhaps the value of the linen-mesh underwear is due to the air which enters into the meshes of the cloth.

Dr. Judson Daland: It is my experience that the majority of people wear too thick woollen underclothing, and therein lies the danger. As regards the kind of bathing we should indulge in, that is largely a matter of individual habit and temperament. The cold plunge of the Englishman is something which he seems to enjoy, while others are made miserable by it. For the past five years I have

looked forward with pleasure to my daily plunge, regulating the temperature of the water according to the weather. Where the reaction is not prompt, I think the cold bath is injurious rather than beneficial. The same rule regarding bathing cannot be applied to all individuals or even to the same individual; even to a person who has grown accustomed to his cold plunge it may prove harmful if he is depressed or has lost his sleep.

Dr. Langmaid: I believe that the skin of some persons has become debilitated, so that it will not properly react, by wearing too much clothing. In children who are too warmly dressed I have frequently noticed the appearance of "goose-flesh" when the skin is exposed, instead of the healthy reaction which should appear. In those who wear linen underwear, or none at all, the skin is in a tonic, healthy state, and there is less danger of taking cold, although the surface of the body may not feel warm.

Dr. Judd: I fully expected a diversity of opinion both as regards the washing and the dressing of the skin. I appreciate it. I have duly considered the subject from all sides. I see no reason to abate in the least the views and practice I have, in my paper, advocated. As to the washing of the skin—that daily process which insures tone and cleanliness—the avoidance of ordinary soaps and harsh rubbings are, in the main, the proper things to observe in the cleansing of the skin. As to the covering of the skin—I do not speak alone from my own experience, but also from that of a large number whom I have induced to make the change—from wool (closely woven) underwear to that of linen (loosely woven) in the manner of a fine mesh. The consensus of opinion is most positive in favor of the latter. I cannot agree with Dr. Daland, who, I am glad to know, believes in it for himself and other so-called "warm-blooded" persons—those of full habit and of robust constitution. Nor with Dr. Babcock, who questions its general utility as underwear under certain climatic and physical conditions. It is alike applicable to the most delicate or to the so-called "cold-blooded" people—those of spare habit with a lowered vitality—in the coldest as well as in the hottest climate. I am not to be understood as saying that there are not natures much more susceptible to climatic changes, and to cold especially, than others. For such there can be no objection to an extra over covering of thin, light wool, if more agreeable, or, better still, cotton and wool mixed. I only claim that while some may feel, as Dr. Walker stated, "a peculiar cold feeling at first" in changing from wool to linen underwear, the reaction comes, and "catching cold" through the change need not be feared. The skin improves in tone, it "wakes up," so to speak, from its inertia, and its functions commence their proper hygienic performance, encouraged and guarded by proper hygienic covering. So a *porous* linen, or even a linen and cotton-

mixed fabric, so long as it is *loosely woven*, *cellular*, or *porous*, is the proper covering next the skin. The reasons are obvious. No clothing is warm in itself; it is only warm in proportion to the air held in its meshes and the dryness of the surface it insures, or to the local climate it creates. A woollen fabric loosely woven, by virtue of its holding capacity, is far warmer than is the same material closely woven. To illustrate, take two bottles of the same size and fill both alike with water at a given temperature; place around one wadding an inch thick, and around the other the same amount compressed to half an inch in thickness, and you will find that the latter will part with heat much more rapidly than the former. When wool is loosely woven it soon shrinks and becomes impervious through the natural moisture of the skin and by washing; thus the air-spaces are lost, and it become positively chilling in consequence, through evaporation, as the moisture is held upon the skin; nor can the skin breathe as it should. The absorptive and drying qualities of wool are the poorest of all fabrics used for skin covering. It is also uncleanly and the most unhygienic possible to conceive, as it retains and holds upon the skin, and in itself, those effete products that should be readily eliminated. Its tendency is also to create a hypersensitiveness and to destroy the tone, as well as to impair the other important functions of the skin. I have spoken of its irritating qualities also. Now, if all this is true as to wool as a skin covering, and if the converse is also true of porous linen properly woven for the same purpose, as in a fine mesh, who could doubt the latter is warmer, healthier, or more cleanly, and altogether the most hygienic in all respects?

There is now made in England and on the market, and, so far as I know, only to be had there, a fabric called "cellular cloth." It comes in cotton mesh, cotton and linen mixed, silk and wool, silk and linen, cellular-fleeced merino—all porous and delightful. I would advocate a selection from such fabrics by those desiring extra *over* covering for greater warmth in extreme weather. I do not doubt they will soon be placed upon the home market. My attention has called to these "cellular fabrics" by Dr. J. L. Heffron, of Syracuse.

The medical profession, as well as the laity, learn best through experience. Up to three years ago I advocated wool as the only proper underwear for all conditions. I wore it myself and ordered it to be worn by my patients. To my surprise and deep chagrin, the closer, thicker, and heavier the wool the more I felt the dread of cold weather and the greater tendency to "taking cold." In midwinter I was utterly incapacitated by a terrible attack of rheumatism. The pain, the sweat, the oppression; the fear of a breath of air, of a touch; the plastering of the skin by exudate, and its retention through the swathing in wool; wool under, over, around the trunk and limbs, housing in effete excreta, excluding fresh air, oxygen—the very essentials to

health and life. All this I experienced personally, and who is there among you that, in his professional life has not been a witness to similar torture, even in this enlightened period? It only proves how the fundamental principles that should govern the practice of medicine are too often overlooked. I then realized the folly and the ignorance displayed in the general treatment of rheumatic cases. Not until my woollen underwear was cut open and removed, skin bathed with alcohol, and thus enabled to breath, did I experience relief. I made the radical change from wool to linen when physically impoverished—"cold blooded." I resumed my work clad in linen mesh. The first day out, in a snow-storm, I felt the cold, but to me it was of a dry, healthy, stimulating nature; not the marrow-searching, deep, chilling kind experienced when wearing woollen underwear. From that day to this I have not experienced a "twinge" of rheumatism; neither do I "take cold" from exposure to the weather, only when I chance to abuse my stomach, and then it rapidly disappears. All this I claim is due to the hygienic care of the skin as I have set forth. I feel that I owe an apology to every man, woman, and child in my practice upon whom I was instrumental in placing wool underwear, whether in health or in disease.

Pardon the length of my debate, but the subject is worthy your serious consideration.

THE CLIMATOLOGY OF NUDITY. (LIGHT, HEAT, ATMOSPHERE.)

By WILLIAM DUFFIELD ROBINSON, M.D.,
PHILADELPHIA.

Physicists and other scientists are getting too far in advance of the medical climatologist in the study of light. By the latter air receives much the greater attention, both in regard to its purity and to its use in health and disease; while it is probably not more important than light. It is only here and there that physicians are beginning to look seriously on that form of energy known as light and to apply therapeutically its proven enormous powers.

Although the use of direct sunlight on the diseased human body is certainly advancing, yet some of the otherwise best papers on the treatment of tuberculosis are defective in not giving any special accent to sunlight; while pure air and altitude are largely dwelt upon. These without light fail, while with light they succeed in curing this disease in its earlier stages. Without light, cultures of tubercle bacilli and many other pathogenic micro-organisms are not unfavorably effected, while with light they speedily die.

Light can cause chemical decomposition and combination with even explosive violence at times, where with its absence no reaction occurs. The higher grade—the more stable compounds—are formed, when the action of the energy called light is a factor.

It may be advisable to enumerate briefly some of the more important physical and chemical properties of light. A universal ether extending through and from the heavenly

bodies into the most minute imaginable spaces in all matter, whether solid, liquid, or gaseous, is accepted as existing. Light is a wave motion of this ether. From about seventy so-called elements all forms of matter are constructed. An atom is the smallest particle into which an element can be divided, and it is in such sized particles that it is related and acts toward the atoms and molecules of all other elements. A molecule is the smallest particle into which matter in combination can be divided without separating the atoms. These are held in some kind of physical apposition by an energy called chemical affinity. Atoms have either negative or positive electric qualities, which are balanced in their union in molecules. The atoms, on entering into a molecule, are not destroyed, but can be reclaimed from the combination in exactly the same state as that in which they entered it.

Atoms of the same element may unite to form molecules just as with atoms of other elements, the mass of molecules resulting being either solid, fluid, or gaseous, according to the ease with which the molecules move about among each other. In gases they repel each other, in liquids they are slightly attracted toward each other, but can move about more or less freely. In solids the holding together is so strong that mechanical force is required to change the physical shape of the mass.

Heat largely controls, whether matter be solid, liquid, or gaseous in form, as it can change one state of aggregation into another in the order named without changing the composition of the molecules.

Light is an undulation or wave motion of ether. Sound is a like motion of molecules in ether. Heat is the motion of molecules of matter in close apposition. Undulations are named waves, as they closely resemble the waves of the sea. In light the vibrations are at right angles to the direction of transmission. The length of a wave of light is measured from the crest of one wave to the crest of the next. Each color of light has a constant wave length. The combination of the waves of all colors makes the so-called common or

white light—that is, light free from color. There is much light named dark light to which the human eye is not sensitive, its wave lengths being too long or too short for the range of the retina. Light waves are sent out from their source in trains of from fifty thousand far upward. Rapidity of travel is constant, making no change for the different colors. Light travels 186,000 miles in a second. This is 1,000,000 times as fast as sound. The average length of a wave of light is one five hundred thousandths of an inch. The various colors of light may be separated by passing it through a prism. The longest wave produces red light, and the shortest violet—an ultra violet. These appear at the respective ends of the spectrum. A prism separates waves, not producing visible light beyond the red end, as far as fifty times the length of the entire spectrum. The length of these waves grows constantly, according to their distance from the visible red. Some have been measured which are thirty times as long as red waves. Beyond the violet end waves of greatly diminished length have been studied. The ultra violet are about one-third as long as the red waves.

The bombardment which we receive from this form of energy is almost inconceivable. The retina of the eye is struck each second by nearly eight hundred (789) millions of millions of waves. The waves are so small, particularly the violet waves, that they pass between and through molecules of matter. The short violet waves and the invisible waves beyond the violet are called actinic or chemical waves, on account of the chemical reaction caused by them. The invisible waves beyond the violet are the most powerful, chemically. It is probably by breaking up, by their dielectrifying power, the electric balance existing in molecules, that waves of light decompose them. Light waves, visible and invisible, have the distinguishing characteristic of being reflected—refracted—absorbed, and polarized. A theory of light now generally accepted by the most advanced scientific scholars is that it is identical with electricity. The velocity of the propagation of electricity waves is the same as that of light waves, namely,

186,000 miles each second. It has been proven that electric waves, although invisible, can be reflected—refracted—and absorbed, just as the waves of ordinary light are. By every test and in every physical property they are the same; but our eyes are not sensitive to waves the length of electrical waves. Electricity is light, or light is electricty, as you choose to put it. Maxwell has proved that light is electric vibration in space. This position is supported by Hertz and most, if not all, of the most advanced students of light and electricity.

Knowing what we do of the longer waves called electricty, and accepting that light is the same form of energy, we can more readily comprehend and theorize as to the probable action of light on our bodies in health and disease. The absorption and later emission of waves of light by many substances has been mentioned, and excites lively interest as to its possible effects on our bodies. Light may be emitted in the same color as that in which it was absorbed or reduced to greater wave length, and emitted in lower colors, calling violet high.

Red and infra-red waves may, after absorption, be given out so lengthened as to be in the lowest form of energy, called heat. Absorbed waves may be retained by some substances a long time—weeks or months—and then emitted as absorbed, when brought into sufficient nearness to susceptible molecules, causing their chemical change.

Just here in our study the following picture may be theoretically painted. A human subject having a deposit of tubercular bacilli in his lungs has his skin freely exposed to direct sunlight, his external skin permitting the light to pass through into the fluids beneath and into the blood of the skin capillaries—the light is absorbed, and is carried along through every minute particle of the body. It is emitted when brought into contact with molecules, the atoms of whose composition are but loosely held together; the destruction of the molecules and of the toxins of the tubercular bacilli, if not the bacilli themselves results, the susceptibility to this

process being the greater by reason of the instability of the molecules of the tubercle and its toxins. Further, the influence of light on the oxygen in the blood and other fluids, creates that more powerfully chemically active form—ozone—which is of great bactericidal power. Also the oxidizing of the toxins of the tubercular bacilli leads to the thought of the future, giving us a new method of selecting and applying drugs and other remedial agents. The chemical stability of the disease-causing agents is dependent on the electric cohesion of the atoms of their molecules. These agents being isolated, as now seems probable, the refinement of matured science may be able to classify and show the most accurate means and methods of electrically rearranging the molecules, so as to be possessed of benign instead of pathogenic powers.

Light being electricity, and granted that various cells of our bodies absorb it, store it, we so become a massive congregation of minute storage batteries, charged from the sun with that wave-length electricity named light. Metabolism may receive its ergo from these, the build-up-and-break-down adjustment in health and disease being maintained by what we name nature. Then we think of immunity and susceptibility being really controlled by firmness and completeness of atomic electric satisfaction.

The penetration of light may be seen by holding the hand between the eye and bright light like a Welsbach or arc lamp. I have elsewhere shown the penetrability of light through various fabrics.

This line of thought could readily be amplified and as well studied with the disease processes of almost any of the other germ diseases. A couple of such may be hurriedly noted. Can it not be that the rash—the blood engorgement of the skin in measles and scarlet fever and erysipelas—is nature's effort toward securing light energy and effective oxygen compounds in the blood in the greatest quantities? They would thus be brought into the closest apposition in the largest volume, and so better combat the organisms and their products.

A possible action of quinine in malaria is interesting. Quinine in solution, as it must be in the blood, absorbs ultra violet and violet waves of light, and speedily emits them in colors. This is called fluorescence. Can quinine, carrying light, thus bring death to the malarial plasmodium and destruction to its toxius? May we not say, carry electrocuting batteries, so named, from light's chemically destructive power, due to its dielectrifying qualities, which extends to action on atoms and molecules. These molecules in the bodies and products of micro-organisms are held in very feeble affinity. Quinine must act by reason of its presence being a medium or carrier of some other energy than that produced by its own destruction, as the salt can be fully recovered from the urine of a subject who has ingested it.

No prominent attention has been called to the fact that light is probably a most important factor in controlling typhoid-fever epidemics. Under its influence pathogenic germs are checked in reproduction and attenuated in virulence. In the recent epidemic of this disease in Philadelphia the cases increased with the continued cloudy weather, but to be reduced to half, and rapidly to less, by a few days of sunshine. The relative death-rate was correspondingly reduced, showing decreased virulence. A new potent therapeutic agent had been added to the treatment. A few well proven results of the action of energy in the form of light waves may be profitable. Carbonic acid (gas) in the leaves of vegetation is decomposed, liberating oxygen. The cloudy seasons of spring and fall are the seasons of germ diseases in the North and South.

Actinic waves bombarding into a glass globe containing mixed hydrogen and chlorine gases cause their chemical union with explosive violence. If the actinic or violet waves are cut off by the interposition of red glass no explosion occurs, but only slow, quiet union. It would seem as though shorter violet waves could possibly pass between the atoms of the respective molecules, composed of hydrogen, to hydrogen, and chlorine to chlorine, freeing the atoms to form

new molecules of hydrogen to chlorine. The energy of these short waves so adjusts the atomic relationship that they loosen their former hold and form the new union, with explosion. As bearing on the power of the nude human skin to absorb all kinds of light waves, I would note that the successful photographing has been reported, in absolute darkness, of a nude subject just after he had been very freely exposed to a copious bath of strong sunlight. Others have reported like results on specially sensitized plates.

Photographic plate manufacturers find that it is necessary to keep opaque glass intended to be coated with sensitized silver emulsion in absolute darkness for prolonged periods, to assure the escape of absorbed actinic waves, or else this light will speedily spoil the plates by reducing the silver to black oxide. The short and poor keeping qualities of celluloid films photographically sensitized is probably due to absorbed light later emitted. Photographic paper keeps badly, not unlikely from the same cause.

A number of cases of lupus have been cured by concentrating sunlight waves on the affected part. Abrams reports success in treatment of laryngeal tubercuIosis, in absorbing furuncles, in treating pityriasis versicolor, cervical lymphadenitis. Time prevents many more possible citations.

An abundance of oxygen, if it could be brought into the electric state for vigorous action, is likely present in the air of almost any of our cities—even in the dwelling-houses—to meet all the requirements for successfully combating tubercular disease. It is an established fact that the successes in the treatment of this disease in sanitoria vary in accordance with the volume of sunlight to which the patients have been subjected. In high altitudes light is more actinic because the chemical waves have not been reduced to longer ones by coming in contact with and being absorbed by the myriads of particles of matter infesting the air at lower altitudes. The possibilities of light as a factor in the health and disease metabolism of our bodies may be far beyond that which we have accredited it. The excessive clothing worn, and the

other environments of civilization, have deprived man of much of the beneficial effect of light.

Accepting light as electricity, only in shorter wave length than electricity as we are accustomed to know it, it will be seen that but a start has been made in a field in which the future may develop much.

I would now ask a minute's attention to that form of energy known as Röntgen or X-rays. It is quite possible that these rays are not any form of light. They cannot be reflected, refracted, absorbed, or polarized. Sunlight and the light of an arc lamp contain none of these X-rays. Sunlight will diselectrify a negatively electrified, polished metal surface, but not a positive electrified one, while X-rays will diselectrify either negative or positive.

Glass is opaque to X-rays, not so to sunlight. There is no proof that X-rays exist as transverse waves in ether, as is the case with light waves, both visible and invisible. Of metals and their compounds, those having the greatest atomic weight are most opaque to X-rays; not so with light.

There is no proof that burns can be produced by X-rays, the cases reported as such being intense electrification of the surface. Investigations by Prof. I. Trowbridge, of Cambridge, seem to conclusively prove this. With over two thousand exposures, by avoiding this electrification no burn has occurred. It has not been proven that X-rays are bactericidal.

My paper being longer than was intended, I will say but little concerning clothing and surface aëration of the body.

It is a law of evolution that any organ will degenerate structurally and in functional power if by artificial means substitution is made for whole or part of the natural functions. We have innumerable verifying examples of this law. The present condition of our skin is one. By almost constant over-covering, day and night for successive generations, the skin has by degeneration adapted itself to its reduced requirements. From birth to senile death we are much over-covered. That a full and vigorously developed skin is a desideratum

will be generally conceded. The tendency is for our skin to degenerate to a tissue paper consistency. The exquisite structure of the skin at once indicates its importance as one of the organs of the body.

A homely showing of that functional power which can be developed in the skin is indicated by the story of the Indian. Being almost naked, and yet apparently quite comfortable in inclement weather, he was asked why he did not seem to suffer and be made ill by the exposure. He replied: "White man's face no pain no sick. Indian all face." By this excessive covering our peripheral nerves are too intensely impressed by caloric changes, our capillary blood system too feebly and incompletely developed to battle most successfully with heat, disease, and traumatic impressions; its muscular fat and connective tissue substance all too deficient and defective for our greatest comfort and welfare. All its functional powers have been reduced. Yet scarcely ever is heard a cry to develop the skin. On the contrary, the trend is toward substitutes which supplant it in function.

HYDROTHERAPY IN THE TREATMENT OF INSOMNIA.

By IRWIN H. HANCE, M.D.,
LAKEWOOD, N. J.

Insomnia, whether observed as a diseased condition unassociated with any other complaint or as an accompanying symptom of neurasthenia, is sufficiently often met with to warrant me in presenting to the members of this Society a short résumé of my past season's work. Insomnia of itself will quickly produce this neurasthenic state, with its long list of vague nervous symptoms, and the physician is sooner or later forced to look for some form of treatment which will combat the two diseases. Most of us know how unreliable drugs are under such circumstances, first because of the uncertainty of securing a definite result, and, second, on account of the great risk run by the patient that he may contract some bad drug-habit, whereby sleep is secured at the time, but the subsequent awakening brings with it the startling realization that he is the victim of one of the enslaving drugs, to escape which he must battle harder than ever man did against the evils resulting from loss of sleep.

"Sleep is a condition of physiological cerebral anæmia." Insomnia, although occasionally met with as a disease *sui generis*, always indicates a disturbed condition of the nerve centres, and, exclusive of sleeplessness in acute diseases, is ususally merely symptomatic of some other disease. In all cases there exists some disturbance of two of the chief systems of the body—the nervous and the circulatory. Therein lies the main indication for treatment, improvement of the cuta-

neous circulation and of the nerve cells. At the same time each case must be carefully inspected for any local cause that may produce reflex nervous conditions, and by appropriate treatment these must be lessened and eradicated, if possible. In so far are drugs and a proper diet powerful agents for good, as will be shown in two of my reported cases. Bearing in mind these things let us see how we can most surely and satisfactorily influence these bad conditions. Even in the milder forms of insomnia the most careful treatment must be pursued, since it is so very difficult to prognosticate the result in this as in all other nervous affections.

To secure the best results it is wisest for the patient to change his environment, allow himself physical and mental rest, exercise out-of-doors only to the point that he is not unduly fatigued, abstain from all varieties of food that produce indigesion, regulate the bowels, for the physician to examine the patient so carefully that no slight disturbance of any one of the organs escapes his attention. Having thus regulated the patient's life to the best of our ability we must seek to restore his deranged nervous equilibrium, if possible entirely without or with the scant use of hypnotic drugs. We have at our command two agents for this purpose—water and electricity. How these are applied, and with what results, I shall proceed to describe by my cases, and afterward draw a few conclusions based on personal observations.

Case I.—Female, single, aged thirty-three years; United States. Insomnia alone. Occupies a position of great responsibility, requiring much mental labor and executive ability. For several months past had been sleeping less and less, until two or three hours was the limit of each night's rest. From lack of sleep was physically weak and in a general run-down condition. Physical examination of all organs and functions healthy. No symptoms of neurasthenia.

January 11, 1899. Treatment: Hot-air bath to perspiration. Needle spray, general fan douche, jet douche along spine, followed by general static electrization (positive), with

local breeze along the spine. Effect of treatment felt after the third bath, sleeping longer and awakening in the morning more refreshed and stronger.

After twelve treatments, sleeping six hours nightly.

27*th*. In New York four nights; slept badly. First night in Lakewood slept seven hours. Continued treatment for two weeks more, and left practically cured five weeks after treatment began. Gave up all work until next fall, and reported two months later cure had been lasting.

CASE II.—Female, married, aged forty-five years. No occupation; suffered from insomnia, more or less, for fifteen years. For five years past distinct neurasthenic with basic headaches, scattered neuralgic pains, tenderness along the spine, marked insomnia, no appetite, very bad circulation.

Treatment: Hot-air bath to perspiration. Needle spray, general fan douche, jet douche along the spine. Took ten treatments. Marked general improvement from the first bath. After the third slept well every night. Gained five pounds. Stopped treatment herself, because she felt that she was cured. Is known to have remained well over two months.

CASE III.—Male, lawyer, aged fifty years. From overwork had gotten into extremely bad neurasthenic state, and slept only two or three hours nightly. Old specific history. Was treated by hydrotherapy and massage last year for a similar condition for six weeks; result good. Stomach examined and excess of hydrochloric acid found. Treated by diet and little medication in the hands of a specialist. Lost fifteen pounds during the fall. Hypochondriacal.

December 29, 1898. Treatment began. Positive electrization. Hot-air bath to perspiration. Circular needle spray, general fan douche, and jet douche to back. At first one to two grains of codeia used to produce sleep. After ten treatments slept seven and a half hours without any sedative. At the end of four weeks slept continuously six to seven and one-half hours.

February 3*d*. Discharged, averaging six hours sleep. Had

gained six pounds. General condition much improved. No longer apprehensive of the future.

Case IV.—Male, merchant, aged thirty-six years, Austrian Jew. Markedly neurasthenic, with depression and a slight melancholia. Suffered from insomnia for six months; averaged three hours sleep or less nightly. Excess of hydrochloric acid in the stomach secretion. Treated by a specialist.

January 16th. Treatment. Four baths given without any effect, then positive electrization was begun and used daily with bath treatment (hot-air bath, circular needle spray, general fan douche, and jet to back.) Little effect upon insomnia during the first two weeks. General nervous condition, however, was improved. During third week sleep improved.

March 11th. Gastric symptoms improved. Resting six hours nightly. Gained three pounds. "Hardly feels any nervousness." Decided to continue his business, which at one time he had intended to give up in order to return to Austria.

Case V.—Male, aged thirty-eight years; Ireland; priest. For ten years had been a sufferer from insomnia. Tried drugs, Kneipp cure, and for three months in the Adirondacks took a bath in cold spring water for ten minutes without friction. Neurasthenic, with mental depression and distinct melancholia. Took ten treatments—hot-air bath, circular needle bath with hot water jet applied to the legs. General fan douche; jet to back; positive electrization. Of the eleven nights during which he was under treatment he enjoyed seven good nights' sleep, and one night slept nine and a half hours. Was obliged to discontinue treatment, and has not been heard from since.

Besides the above reported cases I have had two distinct failures, both in female melancholic neurasthenics, one of whom refused to pursue the bath treatment, and two months later developed violent melancholia. The other pursued the bath treatment alone for four weeks, with slight general improvement, but very little effect upon her insomnia.

In two patients who were treated with the electric baths

and friction, followed by the circular needle bath and the general fan douche, a somnolent condition resulted from each bath; and though the patients were not suffering from insomnia, their rest was more refreshing after the treatment. In some other cases where a hyperæsthetic condition existed the dry or the wet pack was used in place of the hot-air bath, to be followed by the douche-room treatment. Let us for a moment consider what we accomplished by the treatment described, and what deduction we are entitled to draw therefrom.

By the bath treatment there is one thing that is capable of ocular demonstration to every physician, and also the patient himself, viz., an improved cutaneous circulation, as shown by an improved reaction to the eye and the loss of the subjective sensation of cold extremities. With this surface improvement must result a general toning up of the whole circulation and a better action of all the secretory organs of the body. The direct result of this must be a changed condition and bettering of the nerve-cells throughout the cerebrospinal centres as also the vasomotor system, whereby the symptoms which we vaguely ascribe to functional reflex disturbances become weaker and weaker, and finally lose their power for evil. We must not forget another factor, the proper application of which is almost the keynote of success in treating such cases. This is the shock to the nervous system from the change in the temperature and the pressure of the water. The stimulating effect of the shock upon the nerve-centres is felt even in the simplest form of tonic baths, and by slowly increasing this through gently lowering the temperature and raising the pressure we avoid any chilling of the patient, secure his co-operation in the treatment thereby, and by ultimately being enabled to administer a stronger stimulus produce a more decided reaction and more lasting effect.

What part does static electricity take in the ultimate result? I can answer this best by the brief statement that during the first year of my work in hydrotherapy I had not the means

of making use of this agent, and although in two cases of distinct insomnia with neurasthenia I finally secured a general improvement of the patients, and thereby an effect upon their sleeplessness, yet the influence upon the insomnia was slow in manifesting itself. During the second year by hydrotherapy and electricity combined I could see a distinct effect upon this condition in many cases in a few days, and nearly all at the end of two weeks' treatment; and am convinced that hydrotherapy and static electricity together are more powerful in effecting insomnia beneficially than either one is individually.

With these two means it seems to me we are justified in concluding that we are capable of so far restoring the deranged nervous equilibrium, improving the nutrition and resisting powers, toning up the circulation and increasing the muscular power and action of the heart, stimulating the secretory and excretory organs, that nature is capable of adjusting the balance between the demands upon the nervous system and the exhaustion which must necessarily ensue from the constant wear and tear of life in the business and social world of to-day.

DISCUSSION.

Dr. C. C. Ransom: I have not used static electricity in these cases of insomnia, nor hydrotherapy in the manner which Dr. Hance has described, and it seems to me that while the effects of the treatment, as he gives it, are most excellent, he would find a greater field of usefulness for it if the variations in the temperature of the water were not made too great at the beginning. I believe that a very decided reaction should be avoided in neurasthenics. In the douches which I employ in treating cases of insomnia or neurasthenia, I vary the temperature of the water according to the condition of the patient. Neurasthenic patients are apt to be supersensitive, and for that reason a decided reaction should be avoided. My results with the douche have been so universally good in these cases that I have never seen any reason to make any change.

Dr. Hance: In my treatment of these cases the temperature of

the water is carefully regulated, so that all shock is avoided, and therein lies the secret of the success of the treatment of those who are suffering from neurasthenia or insomnia. If the temperature of the water is such as to shock them they will refuse further treatment. In reference to the question of having the water at a certain temperature to-day and another to-morrow, I think that by gradually educating the patients and making a change of two or three degrees daily, you get a quicker result. I look upon that change of temperature as a very essential part of the treatment.

RECENT INQUIRIES CONCERNING THE BLOOD CHANGES INDUCED BY ALTITUDE.

By S. E. SOLLY, M.D.,
COLORADO SPRINGS, COLO.

That there are important changes in the blood when human beings or animals are transferred from an atmosphere in which the air-pressure is normal for sea-level to one in which it is markedly diminished, is admitted by all observers who have studied the subject.

These changes have been found to be practically the same whether the subject of the experiment has been under the influence of diminished air-pressure artificially produced in the laboratory or under the natural conditions in high altitudes. This fact disposes of the theory that the changes are primarily due to the peculiar conditions present in high altitudes other than the diminished air-pressure.

All admit that there is a marked and rapid increase in the number of red cells, and a slower and somewhat less marked increase in the hæmoglobin and specific gravity. While the greater number of the observers believe these changes to be real, there are nevertheless certain others who advance various theories to prove that they are only apparent.

The important point, then, is whether altitude brings about a true blood regeneration or only an apparent increase of the number of red cells and hæmoglobin.

While it is impossible, and indeed unnecessary, to bring before you in detail the literature on the subject, I propose to append to this communication a list of the important references dealing with it, and will chiefly content myself with

giving you an abstract of the most important recent communication. It is by Drs. Ossian, Schaumann, and Emil Rosenquist, of Helsingfors, Finland.[1] These observers conducted their experiments especially with a view to testing the various explanations given of the phenomena.

They confined their observations almost entirely to experiments on animals. The animals (rabbits, dogs, and pigeons) were kept in bell-jars at reduced barometric pressure (450–480 mm. Hg.), according to the method of Regnard, Sellier, and others, for periods varying from nine to thirty-three days.

The blood examination, which was performed in each case at intervals of several days, consisted of (1) count of the red cells (Thoma-Zeiss apparatus); (2) estimation of hæmoglobin (Fleischl); (3) measurement of the diameters of the red cells (dry preparations, average based on 200 to 500 determinations); (4) microscopical examinations with particular reference to the presence of nucleated red cells (Ehrlich's triacid stain, also eosin and hæmatoxylin). The blood was drawn from the ears of dogs and rabbits, from the neck in pigeons. In a few cases blood from the liver and from the aorta was examined before the subject was killed. Besides this the gross changes in the marrow of the long bones was noted in two cases.

In all cases a marked increase (20 to 50 per cent.) occurred in the number of red cells. The high relative humidity (87 to 100 per cent.) in the bell-jars (temperature 21° to 26° C.) makes the assumption of an inspissation impossible.

The increase in hæmoglobin was not proportional to the increase in red cells. A temporary decrease in hæmoglobin occurred in all cases during the first eight to eleven days. A similar temporary decrease took place in the number of red cells in about one-half of the cases.

In order to determine the effect on the return to normal barometric pressure (760 mm.) the blood examination was

[1] Ueber die Natur. d. Blutveränderungen i. Höhenklima. Zeitschr. f. klin. Med., 1898, Band xxxv. Heft 1–4, pp. 126–170 and 315–349.

continued in many cases for from three to ten months after the animals were released from the bell-jars. A decrease in the number of red cells occurred immediately, but was followed by a marked rise, which, after a number of fluctuations, remained in the majority of cases at a decidedly higher figure than was reached by the blood count previous to the experiment. (See tables, pp. 140–149.) The authors think that previous investigations have not been pursued for a sufficiently long period. Leuch's recent work on anæmic[1] school children who had been sent to the mountains, and were examined on their return and at intervals of two to four months afterward, bears out this point (see table, p. 161).

Contrary to all previous investigations, Schaumann and Rosenquist find that during the period of increase of the red cells the average diameter of the cells increases without exception (see tables, pp. 140–149.) This is due to a diminution in the number of microcytes and an increase in the number of macrocytes, the proportion of cells of average diameter remaining unchanged. After a return to normal pressure the average diameter decreases, the decrease occurring in the ratio between the microcytes and macrocytes, while the proportion of cells of average diameter remains the same as before. Measurement of the red cells in blood from one of the authors, made during a journey into the Norwegian Mountains, coincides with this finding (see tables, p. 166). The assertions of Viault, Schroeder, and others in regard to the size of the corpuscles are discredited on the ground that no actual measurements were made, but that reliance was put upon visual comparison. Koeppe's[2] figures in regard to size of cells suffer, in their opinion, from the faults inherent to the hematoerit, and cannot be used for comparisons.

Nucleated red cells were found by them to be slightly increased in number throughout confinement at low pressure, and very markedly at a certain period. This last would seem to have no special significance because no two cases agreed in

[1] Leuch: Correspondenzblatt Schweizer Aertzte, 1896, No. 21, p. 657.
[2] Koeppe: Arch. (Anat.) Physiol., 1895, pp. 154–184.

the time of this period (see pp. 140–149). In the period after the release from the bell-jar the number of the nucleated red cells gradually diminished, except that at the beginning of the diminution of red cells, when a slight increase takes place.

Cell shadows were increased in number after the diminution of red cells began.

The blood of pigeons confined in the bell-jars showed numerous mitotic figures and actual division of nuclei; many of the cells were polychromatophile, which is regarded as evidence of increased proliferation of blood cells.

In specimens of blood taken simultaneously from the skin, liver, and aorta in two animals confined in bell-jars and from two control animals, the number of red cells per c.mm. in each locality was found to be exactly the same.

The examination of the bone marrow gave no reliable results, and no microscopical examination of it is reported.

After reviewing the literature of the subject, Schaumann and Rosenquist point out that six hypotheses have been adduced to account for these changes in the blood produced by high altitudes.

Two of these assume that the increase in red cells is real. Miescher, Egger, and others support the view of increased proliferation of blood cells in the blood-forming tissues, while Fick's theory is that there is prolongation of the life of the individual cell along with a normal proliferation.

The other four hypotheses contend that the increase in red cells is only apparent. Thus Grawitz considers it to be entirely due to an inspissation of the blood, while Bunge believes it is the result of an exudation of plasma into the lymph-spaces of the tissues. Winternitz supposes that red cells become aggregated in certain of the internal organs, and are forced into the general circulation by changes produced upon the latter by altitude; and Zuntz finally refers it to vasomotor control, which is influenced by certain factors of high altitude.

In the light of the results of this investigation the following criticisms of each theory are made. The authors consider

that their results support the theory of new formation of blood cells, but are forced to make changes in the terms of its form.

Vasomotor theories. 1. Zuntz's[1] hypothesis. The authors point out that in their own experiments no factors exist which could give rise to the required nervous irritation; that their animals were removed from the bell-jars for each examination, and that according to the theory the irritation should quickly disappear.

With reference to the theory that the number of red cells is increased in the capillaries and decreased in the larger vessels, it is pointed out that in former investigations blood from both the capillaries and the larger vessels had been examined with the uniform result of an increase in red cells; that the simultaneous increase in red cells and decrease in hæmoglobin (at the beginning of the experiments) cannot be explained by this theory; that a purely vasomotor change should produce no change in the size of the red cells; that the overstimulated nerves would eventually relax; that a return to higher pressure should produce an immediate fall in the number of red cells to normal, which is not the case.

2. Bunge's[2] theory is met with the same objections.

3. Winternitz's[3] theory the authors oppose by reference to their examination in two cases of blood taken simultaneously from the skin, liver, and aorta, in each of which localities which they found the same count (corroborated by Breitrustrin).

4. Grawitz's[4] theory of inspissation. This theory is invalidated by Schaumann and Rosenquist's experiments, in which the respired air was almost saturated with water vapor; by the fact that loss in water by the blood is rapidly compensated for by the tissue fluids, and that a true inspissation of the blood is always accompanied by a proportionate

[1] Schumburg u(?). Zuntz: Pfluges Arch. Physiol., 1896, Band lxiii. pp. 461–494.
[2] Bunge: Verhandlungen d. xiii. Cong. inn. Med., 1895.
[3] Winternitz: Centralblatt f. klin. Med., 1893, Band xiv. No. 49, pp. 1017–1022.
[4] Grawitz, E.: Klin. Pathologie d. Blutes. Berlin, 1896, pp. 333–334.

loss in weight of the animal; and, further, by the fact that in true inspissation of the blood the diameter of the red cells is always decreased.

Theories assuming a true increase in red cells. 1. Fick's[1] theory. This theory, which premises that the absorption of oxygen is lower than normal at high altitudes and the consumption of hæmoglobin is increased, is discredited, because it has been conclusively shown that metabolism is more rapid at high altitudes than at sea-level, and must, therefore, increase particularly the consumption of hæmoglobin.

2. The theory of regeneration of Miescher,[2] Egger,[3] and others is based on the two premises that (1) microcytes appear during the period of increase in red cells, and (2) that the increase in hæmoglobin does not keep pace with that in the number of red cells. The last point the authors grant, and point out that it has been regarded generally (Otto, Hoffmann, and Limbeck) as an evidence of regeneration. The first assumption is disputed, and attention is called to the fact that Ehrlich,[4] Quincke,[5] and v. Limbeck[6] look upon microcytes as products of degeneration of red cells, and also that one of the authors (Schaumann) has found in secondary anæmias that microcytes are most numerous at the height of the disease, and that they disappear as convalescence sets in, and give place to macrocytes.

To determine this point experimentally two animals (a rabbit and a dog) were bled and a differential count made of red cells of various diameters, with the result that microcytes were seen to diminish markedly in number immediately after the bleeding, when regeneration is most active (see table, p. 333.) It was found, moreover, that an increase occurred in the number of macrocytes, and that this, instead of an increase in microcytes, is an accompaniment of regeneration.

[1] Fick, A.: Pflüger's Arch. Physiol., Band lx. pp. 589–593.
[2] Miescher: Correspondenzbl. Schweizer Aertzte, 1893, pp. 809–832.
[3] Egger: Verhandlungen d. 12. Cong. inn. Med., 1893, pp. 262–276.
[4] Ehrlich: Untersuchungen z. Histol. u. klinik. d. Blutes. Berlin, 1891, p. 99.
[5] Quincke: Deutsch. Arch. f. klin. Med., 1877, Band xx, p. 1–31.
[6] Limbeck: R. v. klin. Pathol. d. Blutes, 2 Aufl. p. 207, Jena, 1896.

In accordance with this finding it follows that the increase in macrocytes met with in the blood in the author's first experiments indicates a regeneration of red cells. This conclusion is strengthened by the occurrence of nucleated red cells in the mammals, of mitotic figures in the red cells of the birds employed, and of "cell shadows" in the blood after release from the bell-jar.

Schaumann and Rosenqnist, therefore, conclude that all changes which occur in the blood due to diminution of barometric pressure are best and most easily explained by the assumption that there is an increased proliferation of red cells.

The authors claim that this theory holds also for the explanation of the results of the clinical observations made in high altitudes. They reach this conclusion by a process of elimination, having shown, in their criticism of the similar theories, that causes other than a diminution of atmospheric pressure are insufficient for the production of the hematic phenomena. As positive proofs from clinical material they refer to the following: The hæmoglobin does not increase in proportion to the increase in the number of red cells; the increase in the average diameter of red cells; the presence of normoblast nuclei found free in the blood (the last two points are dependent on the findings in the blood on Schaumann's journey to Norway).

In addition to these inquiries of Schaumann and Rosenquist, some of the most valuable evidence in favor of a true blood regeneration is to be found in Dr. Paul Regnard's book, *La Cure d'Altitude*,[1] in which he gives convincing proofs, based mainly on his experiments at the laboratory of the Sorbonne.

The Smithsonian (Hodgkins') prize memoir for 1898, by Drs. Herrera and Lope, of the City of Mexico, in which the results of their observations conducted on the high plateaux of Mexico are recorded, also support the belief in the true blood regeneration theory.

[1] Masson et Cie, Paris, 1897.

On the other hand, Drs. Meissen and Schroeder, of Hohenhonnef while confirming the fact that there is an increase in the number of red cells and hæmoglobin under diminished air-pressure, ascribe it as largely due to the effect of the diminished air-pressure upon the cover-glass of the Thoma-Zeiss instrument. As, however, the blood examinations of Schaumann and Rosenquist were made after the animals were removed from the bell-jars and under normal air-pressure, as also were those of Regnard and many other experimenters, this theory fails to account for even a proportion of the increase.

The interesting and handsome volume recently published by Professor Angelo Mosso,[1] of Turin, entitled *Man in the High Alps*, contains a protest against the belief in a true blood-cell proliferation, but the only original evidence offered is some experiments made by his assistant, Dr. Kuthy, in Turin, at Gressoney in the Alps, upon two rabbits, one dog, and three men. These experiments are limited to daily observations for the first four continuous days; and therefore, as the blood changes are not complete or permanent under a month, are not worthy of consideration as evidence in this discussion.

I had hoped to be able on this occasion to offer for your consideration the details of experiments that I have instituted in Colorado; but I must postpone this for the present and content myself with merely giving you a brief statement of the general trend of these inquiries. The fact is that there are so many sources of error in the instruments used, and from the variations of the personal equation of the observers, and the variations due to outside causes and circumstances in the observed, that it was found that a considerable amount of preliminary work has to be done before anything like definite figures can be given, so that the experiments conducted in Colorado Springs have been largely an inquiry into the value of the different methods and a study of the

[1] Der Mensch auf den Hochalpen. Angelo Mosso. Leipzig: Verlag Von Veit & Comp., 1899.

causes of error. I am indebted to my colleague, Dr. P. F. Gildea, for the practical work carried on, also to the skilled assistance of Dr. W. Baumgarten and Dr. D. P. Mayhew. Many of the observations were made by these three gentlemen at the same time, consequently the chances of error were considerably lessened.

We believe that to give approximately correct figures numerous observations must be taken and averages made, because two observers counting from the same drop of blood and with the same instrument, within a few minutes of each other, not infrequently find variations in the number of red cells of from 100,000 to 200,000.

Daland's hæmatocrit gave a red-cell count averaging about 500,000 lower than the Thoma-Zeiss instrument. The count in two tubes of the hæmatocrit varied from each other 100,000 to 200,000. When the hæmatocrit was revolved one minute the count nearly approximated that of the Thomas-Zeiss. If revolved two minutes, according to the directions, the count was, as stated, 500,000 lower. If the count was not taken at once it appeared higher because of a rebound in a few seconds from the presssure produced by the revolution of the instrument.

The results from using the Hammerschlag specific gravity test for hæmoglobin varied greatly, and were obviously less reliable than the Fleischl color test. The defect in the Hammerschlag test may have been due to an increased evaporation of the benzol and chloroform under the diminished air-pressure of Colorado.

The experiments, however, confirm in a general way the theory of increased blood regeneration. There was found an increase of the red cells, hæmoglobin, and specific gravity of the blood, after continued residence in Colorado Springs, in the blood of those coming from sea-level. This increase remained permanent during residence, and was found to be only partially lost when the blood was again tested on the return of those who had been anæmic after two or three months' absence at sea-level.

The ascent to higher levels produced a still greater and very rapid increase of red cells. For instance, Mr. B., a gentleman who had kindly submitted himself to frequent blood tests in Colorado Springs, one day made an excursion in a train ascending as high as 10,000 feet, being 4000 feet above Colorado Springs (6000 feet elevation), from whence he started. The following morning, after his return, the blood showed an increase of 600,000 red cells. Several counts were taken later, and the last, a month after making the excursion, showed that he still retained a gain of 400,000 red cells.

I believe the members of the Society could aid materially in solving the problems connected with the influence of altitude upon blood if, when they are sending patients or know of persons in normal health going to Colorado, they would test their blood before leaving sea-level, and would request them to submit themselves to a similar examination immediately after their arrival in Colorado. In this way the contrast would be much clearer than it is when all observations are taken at an altitude and the first examination is made just after the person emerges from the exceptional and trying conditions of a long railroad journey. The members could do good work by testing the blood of persons before and after visiting the seashore. The observations of Marestang[1] are the only ones I am aware of. The effect upon the blood of residence in desert climates of moderate elevation should also be inquired into.

Further, I believe the Society as a body should have a committee who would receive and hold all publications bearing on the subject of climate, and who would send a list of such publications three or four times a year to the members.

[1] Revue de Med., 1890, No. 6.

DISCUSSION.

Dr. E. O. Otis: When I heard Dr. Solly read his exceedingly interesting paper last year before the Philadelphia County Medical Society, I expressed myself then as not convinced from the proof which he adduced that the polycythæmia to which he referred was a genuine one and not limited to the peripheral vessels. To-day I hoped to hear further evidence on this point from the doctor's own investigations in Colorado Springs. I sincerely hope that later on we shall have the complete result of his experiments, which will give us new light upon this most interesting subject. I trust, still I do not feel quite sure, that this is a true polycythæmia. Is it a fact that the whole mass of blood is increased, and are the experiments which have been made thus far conclusive? We have, it is true, the experiments of Egger and his six rabbits. He found this polycythæmia in the blood taken from the deeper vessels, and we have also these further experiments as just narrated by Dr. Solly. Granting the fact that there is an actual increase in the red blood-corpuscles in the whole mass of the blood, how do we explain the fact that there is a rapid destruction of them on returning to the sea-level, without apparently any more signs of general destruction than we know does occur? Why is such a return to the level not accompanied by the symptoms which occur when a similar destruction is the result of ordinary diseases, as in icterus and hæmoglobinuria? Further, is it not a fact that in most cases of tuberculosis the proportion of red cells is normal, and no anæmia exists so far as the blood count informs us, although it, objectively, appears to be present? This whole question is an exceeding important and interesting one in relation to the specific effect of high altitudes. We have still very much to learn of the physiological and therapeutic influence of the climate of high altitudes, and I look for valuable results from Dr. Solly's investigations. But all such study must necessarily be slow and laborious and require the co-operation of investigators at the sea-level in order that comparisons can be made.

Dr. A. Jacobi: There is one point that struck me, namely, the disproportion between the increase of red blood-cells and the hæmoglobin. This, I think, is a very common occurrence, and it should not be attributed to the effects of altitude. We see it continually in convalescents. Yesterday I examined the blood of a boy of seven years, who, six weeks ago, after having been anæmic all winter, had 3,500,000 red cells and from 7000 to 8000 leucocytes, and 60 per cent. of hæmoglobin. His breathing was shallow, but he was fairly comfortable as long as he did not exert himself. Yesterday he had 4,500,000 red cells, and 15,000 leucocytes, a proportion of 1 : 300, and

only 56 per cent of hæmoglobin. The disproportion sets in during a slow convalescence. I have frequently seen the same thing before, and certainly the condition cannot be attributed to the effect of altitude alone.

Dr. Judson Daland: I regret that I did not hear all of Dr. Solly's paper. The first practical point which struck me was the question of the technique of these blood examinations. The work should be as nearly accurate as possible, and in accomplishing this we are confronted with a number of difficult problems. Those men who have been working with blood must confess that, although their methods are the most accurate in existence, they are not sufficiently so to satisfy them. The work now being done in this line is the best we can do under the circumstances, but considerable variation in the results exists, which to some extent vitiates their value. This has probably also been Dr. Solly's experience. For example, in examining the blood of twenty-five persons, all presumably in good health, with the instrument of Thoma-Zeiss, the variation in the count of red corpuscles ranged from 200,000 to 500,000, the count being made from the same drop of blood by two different observers. In making our calculations we count 64 squares, and the number of corpuscles actually counted, in proportion to the whole number contained in the cubic millimetre, is comparatively few, so that a slight error by multiplication amounts to considerable. We observe, also, in using the hæmatokrit that the question of the size of the corpuscles comes in. Each corpusele occupies a certain amount of space, and this again causes inaccuracy. In using Fleischl's hæmometer, the colored dilution of the blood compared with the colored wedge of the glass may cause mistakes. A variation of 5 per cent. or less I consider legitimate. Another point is that the light in the hæmometer should be properly regulated. Very frequently, if the light is strong, we are unable to make a comparison. Still another point is to reduce the area which is under observation, cutting down a large area to a narrow slit, thus minimizing the chance of error; and what we call focussing the eye of the observer upon the wedge-shaped piece of glass deserves consideration, as if you place your eye directly over the narrow slit your count is more apt to be correct.

Whether there is or is not an actual increase in the number of red blood-cells per cubic millimetre resulting from high altitude I do not know, but it seems to me that it is fairly safe to conclude that there is a blood regeneration. The general effect of the climate in high altitudes is usually beneficial; there is an improvement in the general health, and the blood-making process is a part of that We may, therefore, reason from that point of view. In those cases where a diminished quantity of air enters the chest there is a corresponding diminution in the amount of hæmoglobin and the number of red

blood-cells. The red blood-corpuscle is more or less the index of the amount of oxygen required by the body, and we can have a very rapid increase in the number of red blood-cells without a corresponding increase of hæmoglobin. Where we have large volumes of air enter and leave the chest, there is a greater demand on the part of the body for oxygen, and nature's response to that would be the production of a larger amount of hæmoglobin and probably of hæmoglobin carriers.

Dr. Phillips: Assuming that there is an actual increase in the hæmoglobin and in the number of blood cells in elevated climates, the important question is how the increase is brought about. The simple observation of a fact, unless associated with other facts in the relation of cause or effect, merely adds to the burden of knowledge without adding to its usefulness. In addition to the fact, we want an explanation, a hypothesis to account for it.

High altitude climates differ from those of low altitudes in the following respects: The atmospheric pressure is less; the absolute quantities of the constant atmospheric gases are less; the percentage composition unchanged; latitude for latitude, the temperature is lower, the diurnal range greater; the absolute humidity is less, the relative humidity generally so; the number of clear days, and consequently the number of hours of sunshine, is greater in regions protected on the leeward, usually less if unprotected; the chemical intensity of the light is greater, and there is probably a considerable difference in the atmospheric electrification.

What it is important for us to know is, whether the observed effects of elevated climates on the blood are due to the totality or summation of these differences, or to some one or two of them, and if the latter, what they are and by what physiological processes they are accomplished. Thus far it appears that the general tendency has been to regard the diminution of atmospheric pressure as the etiological factor, perhaps slightly assisted by some of the other differences; and some laboratory experiments have been adduced in substantiation of this opinion, and I judge from Dr. Solly's paper he coincides in this opinion. To my mind, the experiments cannot be regarded as complying with the canons of scientific investigation. The reason for this opinion is based upon the fact that the animal experimented on was repeatedly removed from one bell-jar to another, and consequently subjected to frequent, very considerable, though short, changes in atmospheric pressure during such transfers, and also for a further reason that will be mentioned later. Dr. Daland has just told us how many sources of error there are in blood counting—so many, indeed, that we might well be reserved in our acceptance of existent laboratory experiments.

However, I am not disposed to question the reality of the polycy-

thæmia of elevation; I only differ regarding the explanation of the phenomenon. To me there appear even *á priori* reasons for such an increase. It is a notorious observational fact that an organism placed under not too unfavorable conditions strives to accommodate itself to its surroundings, and often in so doing develops functions hitherto dormant or latent. The poverty in oxygen of the air of climates of considerable elevation would naturally suggest that the various functions concerned in oxygen absorption should show an increased activity, with hypertrophy and perhaps hyperplasia of the organs doing the work as a consequence—not, however, as a result of favorable extrinsic influences, but from the intrinsic stimulus of necessity.

Now there has not been advanced either experiment or argument to show that diminished atmospheric pressure was *per se* favorable to oxidation. The contrary, however, has been demonstrated. Diminish the oxygen pressure, and ordinary combustion is less vigorous. Why, then, should oxygenation within the human body be the reverse of that outside.

Accepting the fact that the general trend of all our evidence is to confirm the reality of the polycythæmia of elevated climates, it seems to me that if there be any increase over and above that necessary for the ordinary performance of the normal functions of the blood—in other words, if there is any surplus richness in cells and cell contents —we must look for its causation in something other than decrease of pressure. We should at least see first if the known oxidizing agents present as climatic factors are not capable of accounting for the phenomenon.

Now, two known promoters of oxidation—heat and light—are present in every climate, and the latter especially so in elevated, dry climates. Heat is most active at very high temperatures and but feebly active at ordinary atmospheric temperatures. On the other hand, light is very active at atmospheric temperatures. It has been demonstrated that light increases the physiological elimination of carbon dioxide and the absorption of oxygen—that is, it stimulates metabolism. Light is capable of profoundly affecting the skin, as witness sunburn and also the pigmentation of the skin that follows long exposure to its action, and which is so characteristic of persons leading open-air lives. Who among us has not noticed the invariable increase in ruddiness of the mucous membranes of the conjunctiva and of the oral cavity that results from a few weeks' outing in a person leading a sedentary life? Kronecker and Marti claim to have demonstrated experimentally an increase in the number of red blood cells, and a less degree of increase in hæmoglobin, as a result of exposure of the skin to sunlight and even to the rays of an electric arc light. This action of light in animal physiology finds its parallel in the well-known production of chlorophyll in the vegetable kingdom.

Elevated, dry climates enjoy a larger number of clear days than any other climates, and consequently a greater abundance of sunshine; and it is but natural to expect that one of the peculiar effects of light —that of promoting or inciting chemical activity—should be much in evidence in such climates. It seems to me, therefore, more reasonable to attribute whatever there may be of increased richness in cells and hæmoglobin over and above that needed for normal functionation to the action of the light—an agent that does unquestionably produce such effect in all climates—rather than to some other agent not yet known to possess inherently any such powers in any climate.

Dr. Solly: This is a subject which cannot be compressed within a space of fifteen or twenty minutes. I trust that Dr. Otis will read the literature of the subject, and hope that with his acute, scientific mind he will come to agree with me that there is something in what I have brought out in my paper.

This question of an increase in the number of red blood-cells is one that we consider of special interest in connection with high altitude. Dr. Jacobi stated that he had noticed the same phenomena occurring in convalescents. It is especially in blood regeneration following hemorrhage that this change in the number of red blood-cells is observed, and it justifies the belief in the blood regeneration occurring at high altitudes.

HIGH ALTITUDE AND HEART DISEASE, WITH REPORT OF CASES.

Synopsis :

Report of nine cases of heart disease; a brief statement of the effect of altitude on blood-pressure; mountain sickness, according to Regnard, due to anoxyhæmia; as regards heart disease, the effect of altitude explicable through acceleration of blood-flow; application of this simple theory to the nine cases reported; conclusions.

By ROBERT H. BABCOCK, M.D.,
CHICAGO.

My attention was first attracted to this subject years ago by the following interesting case. In November, 1886, I was consulted by Dr. N., a practising physician in the interior of New York State, who was on his return from Colorado. He was a powerfully built man, of five feet nine inches in height, and weighed 185 pounds. Until October, 1885, he had enjoyed robust health, and prided himself on his strength and feats of physical endurance. He then one day carried a barrel of flour upstairs on his shoulders, and on reaching the top was seized with precordial pain and palpitations. From that time his health failed, a slight dry cough developed, and once he expectorated a little blood. This led him to consult two eminent physicians of New York City, one of whom diagnosticated incipient tuberculosis of the right apex, while the other pronounced the examination negative. Both, however, advised him to go to Denver, which accordingly he did in June, 1886. Upon reaching an altitude of 2000 feet en route thither he began to experience

considerable distress in the chest, and on arriving in Denver, at an altitude of 5300 feet he was, as he stated, almost in convulsions. During his six weeks' stay his dyspnœa and precordial distress were so great that he at length consulted Dr. Denison, who, he said, found his lungs negative, but demonstrated marked dilatation of the right ventricle. The advice was given to seek a lower altitude, and the patient went to South Dakota, where the elevation was about 1500 feet.

His symptoms gradually disappeared, and after a few days he was able to ride horseback without discomfort. This was about the middle of August, prior to his appearance in my office. My records, made after two examinations on successive days, state in brief that the lungs were negative, but that there was a short, rough, presystolic and a faint, blowing systolic apex murmur, with accentuated pulmonic second sound and increase of dulness to the right. The pulse was somewhat accelerated, feeble, and slightly irregular. I made a diagnosis of mitral stenosis and insufficiency, either resulting from or aggravated by the unwise physical effort of the year before, and which accounted for the unexpected ill-effect of high altitude. I have never heard from the patient since that time.

Case II. is that of Dr. W. H. Byford, whom I first examined in February, 1887, making a diagnosis of fatty degeneration chiefly of the left ventricle, due to coronary sclerosis and accompanied by attacks of angina pectoris. In November, 1887, he reported himself as well except for inability to walk rapidly on account of dyspnœa. During the summer of 1889 he made a trip to Alaska, concerning the advisability of which he had consulted me and against which I had urged the necessity of his being subjected to considerable risk in travelling over high mountains on the Canadian Pacific Railroad. Upon his return, however, he reported that at an altitude of 7000 feet he had been able to walk without any discomfort whatever. That serious structural disease existed is attested by the sad fact of his sudden death the following May, 1890, in

an attack of angina pectoris, at the age of seventy-three. No necropsy was made, but during life the somewhat thickened arteries and accentuated clapping, aortic second sound had left no room for doubt of the existence of arterio-sclerosis and resulting myocardial changes.

CASE III.—About ten years ago one of my uncles, then about seventy-two years of age, visited me en route to Denver to pass the summer. His radial arteries felt like a string of beads. He had secondary left ventricle hypertrophy, the heart still being competent. I greatly feared the effect of Denver altitude, and cautioned him accordingly; and yet he subsequently reported that while there he had been able to walk about without the slightest inconvenience. If he had noticed any effect from the altitude it had been for the better.

CASE IV.—Like the one immediately preceding, it is reported from memory, as no notes were ever made. The late Dr. S., of Chicago, whom it was my privilege to know intimately, once drew my attention to a loud mitral, systolic murmur which he said had existed for years, but without particular symptoms. The circumstance that impressed my memory at the time was his comment to the effect that, notwithstanding that murmur, which he regarded as indicative of valvular incompetence, he had just made a trip to Colorado Springs, with an altitude of 6000 feet, where he had been able to walk about with less discomfort than his wife, who, so far as he or anybody else knew, possessed a healthy heart. This lady was rather short, and was, it may be remarked in passing, inclined to corpulence. The doctor at that time was about sixty years of age, I think.

CASE V.—H. P., aged twelve years, was seen in June, 1892, one month subsequent to an operation for appendicitis, during convalescence from which anasarca and other signs of destroyed compensation of a long-standing mitral lesion had appeared. At the date of my visit digitalis and other appropriate treatment had begun to restore cardiac competence, and my opinion was desired upon the advisability of the boy's

return to his home in Trinidad, Colo. At the age of six years, while a resident in that place, he had suffered from acute articular rheumatism, and then for the first time had been found to have a mitral lesion. During the next four years he had been in tolerable health, but had not been able to play with other children because of cardiac symptoms. Two years before my examination he had again suffered from rheumatism with pleuritis and pericarditis, for which the precordia had been blistered. Nevertheless, he had been able to remain at Trinidad, at an elevation of 6000 feet, until his attack of appendicitis. Upon examination I discovered mitral regurgitation, with great secondary dilatation, pleuropericardial adhesions, and old pleuritic adhesions at the base of both lungs, and great visceral engorgement.

Of course, the advice was given not to return to Trinidad, where his tuberculous father was residing. The patient passed the remainder of that summer of 1892 in Canada, and I believe regained such a measure of compensation that during the fall of that year he was taken back to Colorado. I did not see the boy again, but have since learned indirectly that he failed progressively thereafter, and died in about four months from his arrival there.

Case VI.—During the summer of 1894 an attorney, aged forty years, consulted me for an opinion concerning the state of his heart, and, briefly stated, was found to have a pronounced but perfectly compensated insufficiency of the aortic valves. He furnished the following interesting history: He had but shortly before returned from a two weeks' visit in Colorado, where, at the altitude of Denver and Colorado Springs, he had ridden all about on his wheel without any discomfort. One day he attempted the ascent by train of Pike's Peak, which has an altitude of 14,147 feet, I believe. Distress became so great that he was in collapse by the time the summit was reached. The descent was made at once; his consciousness returned before he reached the foot, and when again at the altitude of Colorado Springs he mounted his bicycle and rode away, apparently none the worse for his

foolhardy adventure. I have not seen the patient since my first examination.

Case VII.—At this present writing I have in charge a lady of about forty years, who has been in bed for the last four months in an attempt to preserve from total loss what still remains of a rather inadequate compensation of an extreme aortic stenosis. She has known of her cardiac lesion for the past seven years. Although she had been cautioned against high altitudes, she was yet compelled, about five years ago, to accompany a tuberculous stepson to New Mexico. At first they went to Colorado Springs, where they remained a month. There she was not able to walk more than a few hundred feet without sitting to recover breath. For some reason they then went to Santa Fé, 7000 feet, but remained only two days, because of the great distress occasioned to the lady. In consequence they next travelled to Eddy, in the Pecos Valley, where she was able to stay until the young man's death, two years subsequently. While there, at an elevation of 3000 feet, the lady experienced no particular discomfort, being able to walk about the house and grounds without dyspnœa. She is positive that she has never been as well since her sojourn in New Mexico, and in fact was compelled to seek medical advice for her cardiac asthenia soon thereafter.

Case VIII. is that of a lady, aged sixty-eight years, who had, immediately before consulting me, endured the railway journey from San Francisco to Chicago over the Sierra Nevada and Rocky Mountains. At the summit of the former, 7500 feet, and again at Haggerman Pass, 11,500 feet, she suffered considerably from difficulty in breathing, but at Colorado Springs she experienced no discomfort. Nevertheless, she thought it prudent to sit still in the car and not attempt walking. This patient had a pronounced mitral systolic murmur and great secondary cardiac dilatation, particularly of the right auricle, as shown by the turgid external jugular veins and by percussion. There was evidence also of arterio-sclerosis; and as the pulsations of the thin

walled and probably dilated aorta were visible in the suprasternal fossa and communicated to the distended veins, the erroneous diagnosis had been made of aortic aneurism. Rest in bed, aided by strophanthus and cathartics, lessened the cardiac dilatation markedly and more promptly than I had dared to hope for, and soon thereafter I lost sight of the patient. Six months later she died in charge of a homœopathist, who, I was told, said she developed an ovarian cyst.

Case IX.—Last summer Miss N., aged twenty-one years, was referred to me by Dr. Minor, of Asheville, N. C., because she had not been able to endure the 2500 feet altitude of Asheville, and had to return to her home in Chicago. She stated that she had been forced to leave there on account of the dyspnœa experienced whenever she attempted to do any walking, although such had not been the case when there two years earlier. Examination disclosed free mitral regurgitation, and in addition extensive adhesions between the pericardium and left pleura, as shown by retracted lung border and friction râles on inspiration. The apex was fixed in the sixth left interspace fully two inches outside of the mammary line. The right heart was not bound down. Hepatic and other visceral engorgement was extreme, and disappeared on appropriate treament *pari passu* with the lessening dilatation of the right ventricle; but the ready dilatability of the right heart has its bearing on the patient's inability to endure the moderate altitude of 2500 feet.

Of course, I realize that nine cases afford altogether too slender a basis on which to build a theory regarding the effect of high altitude on the heart and circulation, but they seem to me worth a few minutes' study in the hope that they may aid us in deciding the question whether or not the existence of heart disease always prohibits residence or travel in elevated regions. That such is the case is the general opinion I think—an opinion in which I formerly concurred.

Regnard, on the contrary, thinks that cardiac lesions *per se* do not contraindicate sojourn at high elevations when such residence is necessary; yet he would not advise it, because

there is nothing in their condition of health demanding such climatic treatment. In fact, hæmatosis would be diminished; but aside from this no harm would result from the decreased blood-pressure; and if a cardiac sufferer chose to endure the discomfort he would at first experience until he became acclimated he could do so with impunity. Regnard evidently bases his opinion on his study of the phenomena of mountain sickness, which his experiments have led him to attribute not to diminution of blood-pressure, but to want of sufficient oxygen supplied to the tissues and organs, "l'asphyxie des tissus survient."

Although such may be the explanation of mountain sickness, I nevertheless think Regnard's view regarding the effect, or rather want of permanent effect, on heart patients seems to me too sweeping, as some of my foregoing cases show. There is a quickening of the pulse-rate, and this it is which I think must be reckoned with in considering the question of altitude in cardiac disease.

In an interesting paper in the *London Lancet*, October 15, 1898, Sunderland reports the arrest of menorrhagia at an altitude of 5800 feet, and suggests the explanation that by reason of diminished air pressure on the abdomen and in the lungs the large, distensible veins within the peritoneal cavity become dilated, and thus favor a more rapid return flow of blood from the congested vessels within the pelvis. Unfortunately, this theory is untenable, since it must assume an expansion of the tissues in the walls and organs of the abdomen, and, as Regnard argues, such an expansion of solids and fluids of the body is impossible when the entire surface of the body is subjected to uniform diminution of atmospheric pressure.

It would be far otherwise if the vessels were filled with a gas instead of blood.

Sunderland's explanation is correct only in part. The menorrhagia is arrested in consequence of more active circulation undoubtedly. Yet this acceleration is not due to lessened blood-pressure occasioned by lessened air-pressure,

but I believe to those modifications of the respiratory movements incident to lowered atmospheric pressure.

According to Regnard, the effect of high altitude is to quicken respirations at first and render them more shallow. When, however, the individual has grown accustomed to the diminution of air-pressure, the depth of respiration increases and may even exceed the average. That this increase in the depth and frequency of respiration must accelerate the blood flow will become apparent upon consideration of the effect of respiratory movements on the circulation.

Briefly stated, it may be said that with each inspiration pressure falls within the two venæ cavæ in consequence of their contents being aspirated into the right auricle and ventricle. This latter chamber, therefore, receives and discharges an increased supply of blood into the pulmonary artery, while under the dilating influence of inspiratory expansion of the lungs the flow within the pulmonary vessels is hastened. The consequence is that toward the height of inspiration and during the forepart of expiration a larger volume of blood is discharged into the aorta, and pressure within this vessel rises.

During the latter part of expiration and in the beginning of inspiration, on the contrary, the reverse obtains. If, now, respiratory movements be quickened and deepened, as at high altitudes, the rate of the heart's contractions must undergo a corresponding acceleration. Another factor in hastening the flow of blood to the heart is the action of the diaphragm. According to physiologists (see Kirk's *Physiology*, p. 284), the descent of this muscle, by diminishing the abdominal cavity, causes an upward flow of blood out of the abdominal veins into the inferior vena cava, and thus exerts a pumping action.

It is evident, therefore, that herein lies another by no means insignificant explanation of the beneficial effect of high altitude on Sunderland's cases of menorrhagia.

That an acceleration of the circulation is not merely theoretical is substantiated by the statements of Regnard and

others, who declare that the pulse may reach even 130 or 140 beats to the minute.

Furthermore, Regnard reproduces three sphygmographic tracings which show a diminution of the size as well as increase in the rate of the pulse at an elevation of 1050 metres, while at the summit of Mont Blanc, 4810 metres, the smallness of the pulse becomes still more pronounced. Now, this diminution of the pulse must be due either to engorgement and weakness of the right ventricle, in consequence of which it discharges an abnormally small volume of blood into the pulmonic system, or to the fact that the heart in its entirety handled a small amount of blood with each cardiac cycle. The former hypothesis is untenable, since it is at variance with the effects observed in menorrhagia. Stasis in the right heart would have led ultimately to increased engorgement of the uterine veins and capillaries, and thus have aggravated the condition. Therefore, we must assume that the smallness of the pulse at the summit of Mont Blanc was owing to the rapidity of the cardiac contractions, while its diminution of tension is not due to dilatation of the arteries from lessened air pressure, but to the smallness of the blood wave resulting from the rapidity of the heart beats.

Let us see how this simple explanation can be applied to the foregoing nine cases. Cases II. and III. were instances of arterio-sclerosis without valvular disease, and in both arterial tension was consequently increased. This threw extra work upon the left ventricle, which in Case III. was adequately met by compensatory hypertrophy, while in Case II. the presumably degenerated myocardium was unequal to the task, and dyspnœa on exertion resulted. At 7000 feet, however, the quickened return circulation in the veins relieved pressure in the arteries, the weakened left ventricle found its work easier in consequence, and the patient could walk about without discomfort. For the same reason Case III. was unable to spend the summer in Denver without consciousness of any difference between the altitude there and that of Cleveland, where he resided.

Cases IV. and VI. were instances of regurgitant disease, the one mitral, the other aortic, and both had experienced no difficulty from exercise at an altitude of about 6000 feet. The reason for this seems to me to lie partly in the diminution of arterial tension and partly in the relief felt by the right ventricle. Diminution of pulse tension would tend to lessen the force of the regurgitant stream in either case, and thus lighten the work of the heart. In mitral incompetence the dilatation of the pulmonary vessels and quickening of the flow through them incident to deepened respirations would relieve the right heart, and it is conceivable that dyspnœa might actually be lessened thereby. Since in regurgitant lesions the tendency is for the pulse-rate to be quickened, the heart ought to find but little difficulty in adjusting itself to the acceleration of the venous flow occasioned by lowered atmospheric pressure, and I believe such is the case when complications do not prevent.

In Case VIII. there was free mitral leakage with a greatly distended right auricle, and yet the old lady suffered from dyspnœa and cerebral congestion only at altitudes considerably higher than that of Colorado Springs. This must have been owing to her remaining seated in the car, so that, notwithstanding the more rapid flow into the right auricle, the resistance in front was lessened, as already explained, sufficiently to offset the increased discharge into the auricle. Had she attempted to walk about she would probably have felt very short of breath.

Cases V. and IX. were likewise examples of mitral insufficieney, but complicated by pericardial and pleural adhesions. Here were conditions that would obviously interfere with pulmonary expansion, and would occasion dyspnœa under lowering of air-pressure even without the existence of a valvular defect. There was probably some increased play of the diaphragm which promoted the flow in the inferior vena cava more rapidly than it could be disposed of by the hampered heart. In the young lady's case the adhesions binding down the left ventricle restricted its systole, while

at the same time the unfettered right ventricle was free to receive and discharge its contents until the pulmonary system became surcharged and dyspnœa resulted. In a word, pleural and pericardial adhesions furnish a mechanical hinderance to a proper adjustment of both respiratory and circulatory apparatus to a diminution of atmospheric pressure.

Case I. was an instance of mitral stenosis, and Case VII. of aortic constriction. In both an unyielding barrier to the blood flow existed, and time had to be allowed for the stream to pass the point of obstruction. In stenosis, systole is slow and forcible so long as compensation is adequate. Acceleration of the circulation at high altitudes would quicken cardiac contractions; with the shortening of systole time would not be allowed for complete emptying of the chambers back of the constriction; stasis in the pulmonary vessels would be promoted, with corresponding dyspnœa, that might readily grow urgent on exercise.

Conclusions. 1. All forms of cardiac disease do not contraindicate sojourn at a high altitude.

2. The ill-effects of low atmospheric pressure in some forms of cardiac disease are explicable on the hypothesis of acceleration of venous flow and corresponding quickening of the heart beats.

3. Consequently those forms with which high altitude is likely to prove incompatible are pronounced aortic or mitral stenosis, and regurgitant disease when complicated by pleural and pericardial adhesions.

4. On the other hand, patients with uncomplicated regurgitant lesions, or arterio-sclerosis with or without myocardial changes, may endure low atmospheric pressure without injury.

DISCUSSION.

Dr. Henry Sewall, of Denver: All discussions as to the effect of altitude in heart disease are based on theories of the mechanics of the circulation which are not yet firmly established. So far from a moderate altitude, as that of Denver, about one mile above sea-level, being a disadvantage in some cases of valvular heart disease, it has seemed to me to be distinctly beneficial. This conclusion is based upon the clinical observation of diseased hearts, and strengthened by the changes occurring in weak but structurally normal hearts, recently removed to Denver from lower levels. In these cases, usually sufferers from pulmonary tuberculosis, there is manifested soon after arrival, in my experience, more or less cardiac disturbance attributable to overstrain of the right side of the heart. This is the period when muscular exercise is apt to work greatest injury to the patient. In cases that do well the signs of heart strain pass off after a few days or weeks. The phenomenon is readily explained if we admit that with removal to higher altitude the aspirating power of the thorax is increased by augmentation of the depth and frequency of inspiratory movement. In the well-nourished heart there probably occurs a compensatory hypertrophy of the right side, which enables it to discharge easily the increased intake of blood, and in turn subjects the lungs to more vigorous irrigation.

In cases of mitral stenosis, the observation of which forms the basis of Dr. Babcock's paper, there is already a characteristic excessive burden thrown upon the right heart, and from the theoretical stand-point it would seem bad practice to advise an environment in which the flow of blood into the right ventricle would be accelerated. From the clinical point of view my experience leads me to the same conclusion, and I feel grateful to Dr. Babcock in having so clearly formulated the mechanical conditions of disease under which the heart is most apt to work at a disadvantage in higher altitudes. On recalling those cases of heart disease that I have seen go to ground in Denver without sufficient reason, it has been found that the signs of mitral stenosis characterized the greater number.

On the other hand, it seems clear that the condition of cardiac nutrition is in the end the factor on which the heart must depend for its powers of compensation, and the means by which an improvement in this nutrition is to be gained are more to be sought than the conditions which lead to increase of burden on the heart are to be feared. Physiological experiment has demonstrated that the heart is almost unique in its tolerance of mechanical injury, but with defective metabolism its powers begin at once to fail.

It is stated on good authority that a sudden fall in the specific gravity of the blood is the immediate precursor of loss of compensation in heart disease. The anæmia in this condition is one of its characteristic features. The literature supporting the view that high altitudes cause an increase in the red corpuscles of the blood is well known. My colleague, Dr. W. H. Bergtold, has made a careful comparative study of the specific gravity of the blood, and finds that the specific gravity is characteristically higher in Denver than at sea-level. An influence of residence in moderately high altitudes, which seems to me of unrivalled importance, if somewhat mysterious in its mode of action, is that manifested in the psychical attitude. The sense of *well-being*, physical and mental, is apt to be improved. No one will dispute the importance of the psychical factor in heart disease. The facts seem to me to warrant the conclusion that moderately high altitudes favorably affect cardiac metabolism, but throw on the heart, especially the right heart, an increase of burden. So long as the nutritive powers of the heart-tissue are normal such altitudes are favorable, because while not overburdening the heart they facilitate through it the nutritive changes in other vital tissues. But so soon as compensation fails from defects which can be attributed to retrogression of cardiac tissue, the dangers of the increase of the physiological burdens incident to high altitude far overbalance the benefits of its stimulus.

Permit me to close with reference to a female patient with marked mitral stenosis who, about a year ago, came to Denver from Chicago, where she had been under the careful observation of Dr. Babcock. I questioned her carefully as to the effects of different altitudes upon her condition. She said she could stand Colorado Springs, with its altitude of 6000 feet, with only fair comfort. In Denver, 5280 feet, she was just as comfortable as in Chicago, and better than in Los Angeles. But in Cripple Creek, 9300 feet, she at once began to feel miserable, and at the end of a week would break down completely. Two months ago I saw the patient through an attack which had come upon her in Cripple Creek. She soon after returned to her normal condition, in which she was able to take a fair amount of exercise with comparative comfort.

Dr. Charles E. Quimby, of New York: I take a peculiar interest in Dr. Babcock's report of the effect of altitude upon cardiac disease, because for seven years I have been treating all forms of valvular and mural disease of the left heart, with the exception of mitral stenosis, by pneumatic differentiation. There is no other therapeutic measure whose action is so definite and constant, or the effects of which can be foretold with such accuracy. The laws of aërodynamics and hydro-dynamics have been established and fixed for years. The effects of atmospheric forces upon other forces are equally

certain and definitely formulated. Of course, we recognize that results with this, as with all therapeutic measures, depend quite as much upon the responsiveness of the tissues as upon the agent employed. Yet that factor is no more important in this than with all methods. While it is doubtless essentially true that liquids are incompressible, nevertheless, in the treatment of cardiac disease by whatever method, we are forced to recognize a force that is essentially developed by blood compression. When we speak of blood-pressure as a certain number of millimetres of mercury, we really mean that pressure in excess of atmospheric pressure. In other words, the blood is under a compression of nominally fifteen pounds to the square inch in excess of what we consider and measure by the mercurial gauge. It is this latent force normally counterbalancing atmospheric pressure that becomes active with the decrease of atmospheric pressure at high altitudes and which is made dynamic by pneumatic differentiation. Since blood-pressure is thereby lowered, the value of final results depends upon the possibility of maintaining or increasing tissue nutrition under lowered blood-pressure. Formerly we were led to believe from the teachings of Cohnheim, that tissue metabolism varied directly with blood-pressure. I have been convinced by the results of treatment in cardiac disease by pneumatic differentiation that such is not the case, or, rather, need not be the case. It is true that the system has no other resource for hastening blood flow than by increasing blood-pressure, and we admit that when the circulation is not under the influence of external forces nutrition does vary as blood-pressure. But I am confident that my results in cardiac and pulmonary cases during the past seven years prove beyond question that with augmented blood flow tissue metabolism may not only be maintained, but even increased under a lowered blood tension. The additional proof of this which Dr. Babcock's cases afford, is the cause of my peculiar interest.

Dr. Babcock: I am very sorry that I did not hear more from our Colorado contingent, for I began to be very shaky about the correctness of my theory. Practical experience is worth more than theory, and these men have all had experience with cardiac patients which might entirely upset the theory that the rapidity of the circulation was the cause of the disturbance of the heart in certain cases. Particularly was I overwhelmed last night when Dr. Sewall informed me that one of my patients with pronounced mitral stenosis was walking about Denver in comparative comfort. Nevertheless, I think that, as a rule, the principle can be laid down that mitral or aortic stenosis should be regarded as contraindications to a residence in high climates; also, when there are complications, such as pleuritic adhesions. One individual with a certain degree of mitral stenosis can endure the altitude of Denver, while another with a similar lesion

cannot endure a much lower altitude, and it is hard for us to explain this. The explanation will probably be found in some change in the myocardium, perhaps a degeneration, which, when coexisting with valvular disease, will determine whether or not patients can sojourn in certain altitudes. My paper was simply offered in the way of a suggestion, and in the hope that it would stimulate the members of this Association to report more case of this kind, and to lead them to realize that the mere existence of cardiac disease does not, *per se*, contraindicate a sojourn in high climates.

PROGNOSIS IN CHRONIC VALVULAR AFFECTIONS OF THE HEART.[1]

By N. S. DAVIS, Jr., A.M., M.D.,
CHICAGO.

Most laymen anticipate sudden death when they learn that they have heart disease.[1] Such was the expectation of physicians in the first part of this century. It is now known that sudden death from chronic valvular disease of the heart is the exception, not the rule. It is to be looked for only in cases of aortic regurgitation. According to Broadbent's statistics, death occurs suddenly in about one-quarter of these cases. One sudden death also occurred in eleven of his cases of aortic stenosis. According to other observers it is extremely rare in the lesion of stenosis, and is almost unknown in mitral lesions.

By sudden death is meant death instantly, without apparent aggravation of symptoms or within an hour or two after the fatal symptoms manifested themselves. When angina pectoris complicates aortic regurgitation the danger of sudden loss of life is much increased. If the valvular lesion develops in one who has general arterio-fibrosis a sudden demise is much more probable than if the lesion has been produced by rheumatism or an infection. In other words, sudden death in these cases is not due as much to the valvular lesion as to the very frequent coincident pathological state of the coronary arteries or coronary circulation.

When a valvular lesion actually exists perfect recovery is

[1] While preparing this paper the records of 250 cases observed in my private practice have been studied, and the statistics quoted have been obtained from this source.

possible. Anatomieal recovery is rare, in a sense is impossible, but physiological recovery is not uncommon. If a murmur is constantly heard in one of the valve areas on the chest an anatomical defect exists, although the heart is not demonstrably changed in size, performs its functions perfectly, and the general condition of the patient is one of health. If a murmur which has been persistent for weeks, and has been associated with hypertrophy of the heart, disappears and the hypertrophy gradually lessens, the heart ean be said to have recovered both physiologically and anatomically. Such apparent anatomical recovery is usually due to a slow stretching of one valve curtain to compensate for a slight defect in another. It very rarely happens, if it ever does, that the chronic thickening, roughening, or contraction of a diseased valve disappears entirely ; therefore, even when clinical evidences point to an anatomical recovery, it cannot be said to be perfect.

My own experienee with cases of apparent anatomical recovery makes me skeptical of its completeness and permanence. I have observed it twice in cases of mitral regurgitation of rheumatic origin. These patients were under observation for several years, and it was found that whenever the heart's rate was increased from transient and moderate fever or violent work the murmur would temporarily reappear. A third case, now under observation, came to me with broken compensation and mitral regurgitation of moderate extent. As compensation was improved the murmur grew fainter. When the patient was last examined it was inaudible except as arterial pressure was increased by raising his arms or causing him to make hurried movements.

Apparent anatomical recovery only occurs in valvular insufficiency. It has been observed both in aortic and mitral affections. That this must be so becomes apparent when the nature of the lesions producing stenosis is recalled. A narrower orifice does not stretch, and cannot be compensated for. If stenosis is due in part to protruding vegetations these may break off, and thereby lessen the degree of stenosis, providing

they do not then cause fatal embolism. Moreover, stenosis is apt to be a progressive lesion.

Germain See is quoted as declaring that he had never seen aortic lesions perfectly recovered from. Mitral insufficiency he believed incurable; but mitral stenosis, even when it was accompanied by persisting dyspnœa and œdema, he had seen improve, and ultimately physiological recovery become perfect. As regards aortic lesions, my experience accords with this statement. I do not now recall a case of aortic disease in which rapid beating of the heart, dyspnœa on exertion, or other evidence of lack of compensation did not exist; but I have seen most perfect compensation for both mitral lesions.

The chance of moderate—one cannot say of average—longevity is good in chronic valvular disease, providing compensation is perfect. There are, however, certain symptoms and conditions which make it possible to predict the relative duration of compensation. It is generally said that if the lesion is one of mitral insufficiency the chances of longevity are greatest, and are relatively less for the other valvular lesions in the following order—mitral stenosis, aortic stenosis, aortic insufficiency. Walsh and others reverse the order of mitral and aortic stenosis.

Of thirty fatal cases of which I have records, the average age at death was fifty years for mitral stenosis, forty for mitral insufficiency, and thirty-six for both aortic stenosis and insufficiency.

Stillman[1] says the percentage of deaths from heart disease among 5000 insured persons is 1.84 between eighteen and twenty-nine years; 3.97 between thirty and thirty-nine; 5.1 between forty and forty-nine; 8.9 between fifty and fifty-nine; 13.3 between sixty and sixty-nine, and 12.6 between seventy and seventy-nine years. The progressive increase in mortality is strikingly noticeable.

Of those of my own cases of which the duration of compensation could be determined with reasonable accuracy it

[1] C. F. Stillman: The Life Insurance Examiner, p. 30.

was found that in mitral insufficiency it averaged 5.1 years ; in mitral stenosis, 11.5 years ; in aortic stenosis, 7 years, and aortic insufficiency to 2.3 years. The range of duration in thirty-one cases of mitral insufficiency was from three months to seventeen years. Seven cases lived with good compensation one year; five, two years; four, three years; three, six years ; four, ten years. In mitral stenosis the range was from six months to twenty years ; three cases lived ten years with good compensation. In aortic stenosis the range was from two years to twenty, and in aortic insufficiency from one to four. From these statistics it appears that the chances of prolonged compensation and longevity are greatest in mitral stenosis.

The next factor which must influence prognosis in these cases is the extent of the lesion. This can be imperfectly estimated. When a murmur is systolic and the first sound of the heart is also recognizable, the valves are able to do better work than if only a murmur is audible. If in mitral insufficiency the first sound is prolonged into a murmur, it is evident that at first the valves are nearly or quite closed, but are forced asunder later when the intra-ventricular pressure is highest. Whenever a murmur only is audible in a given valve-area, and the other cardiac sound is inaudible, the lesion is most extensive. A loud and widely diffused murmur indicates strength in the heart-muscle. On the other hand, a low murmur need not indicate weakness. It sometimes means that the lesion is not extensive. A long murmur usually signifies a comparatively trivial lesion or one just formed. When either of the heart-sounds has been accented, and the intensification of sound lessens, a loss of muscular strength has occurred.

Another factor to influence prognosis is the stationary or progressive character of the lesion. Congenital lesions are stationary. Those produced by endocarditis resulting from measles, scarlet fever, and other eruptive diseases are stationary. Chorea without marked rheumatic symptoms produces comparatively favorable cardiac lesions. Rheumatism is so apt to occur, and almost without fail to aggravate cardiac

lesions at each recurrence, that chronic valvular disease of rheumatic origin must commonly be regarded as intermittently progressive. Lesions which develop as a part of degenerations, such as arterio-sclerosis and atheroma of the aorta, are progressive ones, and make prognosis least favorable.

Since the degenerative lesions develop only after middle life, the age of a patient is a partial guide as to his probable longevity.

Although the valvular defects just described as stationary are so, the cardiac disease growing out of a mitral stenosis, even in childhood, produced by an eruptive fever or mild chorea, is likely to be progressive, for while the child grows the left ventricle, aorta, and arteries generally do not enlarge as they should, and sooner or later, by their relative small size, become an additional impediment to the circulation. The right ventricle is then unable to maintain compensation.

It is interesting here to observe the time of inception of the various valve lesions. Of sixty cases of which the time of inception could be determined forty-nine began, before the fortieth year, in mitral insufficiency; in mitral stenosis the ratio of those beginning before the fortieth year to those beginning later was as 10 to 2; in aortic stenosis they were equally divided by the fortieth year; in aortic insufficency, on the other hand, the ratio of those beginning before the fortieth year to those after was as 2 to 7. The greater age at which aortic maladies have their inception is also well shown by the fact that in these same cases mitral insufficiency was observed to originate from the third to the sixty-ninth year; mitral stenosis from the sixth to the sixty-seventh year; aortic stenosis from the twelfth to the sixty-second, and aortic insufficiency from the twentieth to the seventieth.

A fourth factor which will modify prognosis in cases with good compensation is the amount of hypertrophy that exists. When compensation is imperfect without demonstrable hypertrophy or only trifling hypertrophy, the chance of longevity is comparatively good, for it is possible that compensating

hypertrophy will develop. If hypertrophy must be great in order to maintain compensation trivial changes will break it.

The state of general metabolism is a good guide as to the endurance and longevity of the heart. If a patient's muscles are firm, his blood rich, and his weight maintained, the possibility of prolonged compensation is good. Whenever considerable losses or gains in weight occur, either rapidly or progressively, the danger of cardiac weakness increases. An approach to obesity is almost sure to break compensation. A considerable loss of flesh is usually due to general malnutrition, which almost uniformly causes cardiac weakness.

The habits of a patient may increase the chances of breaking compensation If, to maintain himself or family, violent physical exertion must be made, or moderate exertion continuously over many hours, cardiac exhaustion must be looked for soon. Glaring exceptions to this rule can be pointed out, but it is nevertheless a general statement that can be maintained. If one's mode of life disposes to exposure, to wet and cold, the danger of complications, such as bronchitis, pneumonia, nephritis, and rheumatism is great. These maladies are often the cause of a break in compensation, and if they recur several times are sure to cause it. The frequent or constant use of unwholesome or indigestible foods causes digestive disorders, which will increase blood-pressure and the heart's work. Alcoholics are liable to provoke gastritis and consequent indigestion and higher arterial tension. They are also a cause of degeneration of the heart, and when freely used tend to produce cardiac dilatation.

Chronic valvular lesions which are not progressive and are produced in early adult life are least dangerous. As they originate nearer either extreme of life they lessen the chances of longevity.

It is impossible to estimate in weeks or months or years the length of life of individuals who suffer from any particular form of valvular heart disease. It is only possible to enumerate the number of favorable and unfavorable factors in a given case, and from them to estimate the chances of

prolonged life. Broadbent has said that those suffering from aortic disease lived, on the average, four years after they were first admitted to hospitals for the cardiac trouble. This must mean four years after compensation was lost, if it ever existed.

Of one hundred and three of my cases of which the duration of broken compensation could be determined (I do not mean the entire duration, for many of them were not under observation up to the time of their death) it appears that in mitral insufficiency the average duration of broken compensation is at least 2.6 years, with a range of from six weeks to seventeen years; in mitral stenosis the average is 3.6 years, with a range of from six months to eleven years; in aortic stenosis the average is 3.8 years, with a range from three months to twenty years, and in aortic insufficiency the average is 2.75 years, with a range of from six months to seven years. This shows that after compensation is broken those having stenosis of the mitral or aortic valves have a greater average duration of life than those having insufficiency.

Cases in which compensation is broken can be placed in three groups, according as compensation is imperfect; lost and characterized by œdema; or more than lost and complicated by passive congestion of the liver or kidneys, œdema of pleura, pericardium or lungs, or by deep anæmia. Individuals in the first class may average a life of three to four years; those of the second class one of six to eighteen months, and those of the third some weeks.

When compensation is broken a quick pulse and more or less dyspnœa on exertion coexist. Often coughing occurs also, and sometimes œdema; but of these symptoms dyspnœa on exertion is oftenest the first to attract the patient's attention. Of one hundred and five of my cases sixty-two developed dyspnœa as the first noticeable evidence of broken compensation, thirty-three a quickened heart's action, six œdema, and four cough. The contrast between the first symptoms of broken compensation observed by patients in mitral and aortic lesions is shown by these ratios. In mitral lesions dyspnœa was first observed three times oftener than rapid

beating of the heart. In aortic stenosis a quickened heart-beat was noticed four and one-half times oftener than dyspnœa, and in aortic insufficiency it was uniformly the first symptom of broken compensation.

In all cases of ruptured compensation dilatation of the ventricles is the rule. Dilatation is dangerous in proportion as it exceeds hypertrophy rather than directly as the ventricles are enlarged.

The danger which dilatation of the heart causes is much greater when it is chronic than when it is acute. Indeed, the latter is commonly recovered from.

The ability of a dilated and hypertrophied heart to do its work is best gauged by the fulness and strength of the radial pulse or by the degree of persistent dyspnœa which may exist.

Very considerable passive congestion of the liver and kidneys indicates a permanent loss of compensation. It is true that we are often able to restore these organs to a natural size, and even to do so several times, but we cannot produce again cardiac compensation.

When compensation is lost, deep anæmia, even if dropsy is not considerable or the heart greatly dilated, makes prognosis most unfavorable, for necessarily it means that the heart, as well as all other tissues, is imperfectly nourished, and therefore that exhaustion is imminent; but anæmia complicating chronic valvular disease may be cured before compensation is lost. It may be lessened or temporarily cured when the heart first shows signs of permanent weariness, but it can rarely be much changed when cardiac exhaustion is established.

After compensation is broken patients usually are watchful of their health, and seek and heed advice. They modify their habits, as far as possible, to meet the requirements of their malady. Habits, therefore, at this stage of the disease's progress are of less importance than disposition. The heedless and headstrong patient is sure to be indiscreet, and the ignorant one will often unwittingly work when he should not, or discontinue treatment when it is doing him the most good.

The habitual use of tobacco by those having heart disease is detrimental. If used freely it hastens and increases cardiac dilatation and degeneration. If used in moderate quantities its ill effects are usually evident.

In all forms of chronic valvular diseases chronic indigestion and constipation are a menace; on the one hand, to the duration of compensation; on the other, to the remaining strength of an exhausted heart. They increase arterial tension, and therefore the work which the heart has to do. These conditions should be promptly corrected or at least mitigated.

Chronic valvular diseases are said to be commoner in women than in men, but less rapidly fatal in them.

Pregnancy and childbirth increase very greatly a woman's danger when the valves of her heart are diseased. I presume we all can recall mothers who have borne and reared families although anatomical defects existed in the valves of their heart; but after compensation is broken childbearing is a grave complication. I have never seen a case in which it did not shorten life, and it has been my misfortune to see several in which it caused death immediately or rapidly.

Rupture of a valve produces an acute rather than a chronic valvular lesion, and is not properly included under the title at the head of this paper. This lesion causes immediate loss of compensation, and death follows usually in a few weeks, and sometimes in a few days.

Out of one hundred and thirty-nine of my cases rheumatism was the undoubted cause in fifty-four, chorea in five, atheroma in fifteen, la grippe in four, pneumonia in three, scarlet fever was a possible cause in three cases, ague in three, puerperal fever in one, measles in two, typhoid in two, and trauma in one. Rheumatism was the cause of mitral insufficiency, mitral stenosis, aortic stenosis, and aortic insufficiency with the following degrees of frequency: as 8 is to 4, to 3, to 2, respectively. In only three cases were mitral lesions alone observed to apparently arise from atheroma, but they were several more times associated with aortic lesions.

Combined aortic stenosis and mitral stenosis were observed three times; aortic stenosis and insufficiency five times; mitral stenosis and aortic insufficiency two times; mitral insufficiency, and aortic insufficiency one time; aortic stenosis, insufficiency, and mitral stenosis one time; and both aortic lesions and mitral insufficiency two times. Incompetence of the auriculo-ventricular valve of the right heart was observed three times associated with mitral lesions.

Bronchitis was the commonest complication observed. Nephritis occurred very often, but six times in aortic lesions to four in mitral. Anæmia and asthma were also common complications. Tubercular disease of the lungs occurred in three cases, pericarditis in five, angina pectoris in five. Rheumatism recurred often in those cases caused by it. La grippe and gastritis were also occasional complications in these cases of chronic valvular disease of the heart.

From the stand-point of prognosis it is interesting to know that the mortality from heart-disease varies in different sections of this country. In 10,000 insured persons in each of the following districts the mortality was as follows: New England and New York, 6.1; Michigan, Wisconsin, Minnesota, Nebraska, 4.1; New Jersey, Pennsylvania, 7.1; Ohio, Indiana, Illinois, 4.4; Delaware, Maryland, District of Columbia, Virginia, Kentucky, Missouri, 6.3; Southern States, 6.6; Washington, Oregon, California, Utah, Dakota, New Mexico, 7.4. (Stillman's *The Life Insurance Examiner*, p. 12, Appendix.)

I had intended to cite cases to illustrate many of the varying conditions which must govern prognosis in chronic valvular diseases of the heart, but the restrictions placed upon the length of papers to be read here has caused me to omit them.

DISCUSSION.

Dr. Roland G. Curtin: There is an important question bearing on the subject of prognosticating heart disease that I desire to speak of at this time.

The framers of our "Code of Ethics" were very wise when they insisted that a positively unfavorable diagnosis should never be made, and I think we should exercise great care, especially in these cases, in not being too positive, even after a careful examination has been made and we are sure in our own minds of our diagnosis. From a neglect of this rule I know of lives that have been ruined when the error was not only in prognosis, but also in diagnosis. To illustrate my point I will cite two cases that have lately come under my observation.

I was called in consultation by a doctor to see a brother of his who gave the following history:

At the time of his death he was thirty-three years old; had never been very strong, but was a steady man, with good habits and instincts. When he was twenty-two he went with four of his friends to be examined for life insurance. His friends were all accepted, but the examiner rejected him on the ground that he had a serious affection of the heart, which he told him would probably prove fatal within a year. He became mentally depressed, and soon began to drink in order to drown his fear of death. It was nine years later that I saw him, and he was then a confirmed alcoholic, with influenza, pneumonia, and heart failure. Lips, nails, and hands blue; pupils dilated, and muttering delirium. He had a mitral systolic murmur, but it was not very marked. The curious part of this case was that all four of his friends and the doctor who had been insured had died before he did.

Another case is that of Mr. J., now about sixty years old. Forty years ago he was told by one of Philadelphia's most noted heart specialists that he had a dangerous form of heart disease. Since that time he has never worked or indulged in any exercise, and is so nervous that he is afraid to be left alone, and yet no one can discover that he has any evidence of cardiac or any other disease.

I think everyone will agree with me in saying that it is surprising the improvement that occurs in some cardiac cases that at first sight were apparently hopeless.

Dr. James B. Walker: I wish to report one case which has a bearing on prognosis in heart lesions. Ten years ago I was called to see a young girl, aged fourteen years, who had suffered from acute articular rheumatism and showed signs of dropsy, which was attributed by her previous physician, a homœopath, to suppressed menses.

She was brought to Philadelphia and placed in the care of a noude-script, who had the same idea. I was hurriedly sent for one night, as the girl was supposed to be dying. I found her cold as marble, and pulseless at the wrist, with extreme general anasarca, the patient being in advanced dropsy, pleuritic and peritonitic effusions, albuminuria. The mitral insufficiency which had long existed had developed into almost complete incompetency. Under appropriate treatment the patient gradually emerged from her condition, and by the following morning I felt fairly hopeful of her recovery. A leading consultant who saw her with me said she would not live to go back to her home in Maryland. Two days later the patient was well enough to be up and open the door for us when we made our morning call. Her dropsy had almost entirely disappeared, the urine had cleared up, and the girl went back to her home in three weeks, to all appearances well, but with, of course, the heart lesion. She was able to play tennis and take her part in all out-of-door games at college, where she graduated. She is now twenty-four years old, is married, and is enjoying excellent health. Her mitral regurgitant murmur is still present, and will accompany her through life; but for ten years the crippled heart has been rendered so completely competent that no one has suspected her to be invalided in any way. For a severe case, where competence was at one time lost, her case is the most remarkable I have seen, and is the best argument against unfavorable prognosis in cardiac valvular lesions my experience has had presented to it.

Dr. Babcock: To me, statistics upon the length of life in the various groups of valvular lesions have never had much value, because the cases are not grouped quite in the way they should be. The cases we heard cited to-day were grouped before and after the fortieth year, and cases of mitral insufficiency occurring before and after that age were compared. In order to have any value, valvular lesions occurring after middle life should be compared with each other. A valvular lesion occurring before middle life or beginning in childhood should be compared with one of the same character. Furthermore, lesions having the same etiology should be compared with each other. Cases of aortic insufficiency beginning after middle life cannot have the same prognosis as these cases beginning, say, at the age of twenty from an endocarditis. Although I recognize that certain general principles can be laid down concerning the length of life, still the individual factor in each case must be considered. All these points have been touched on in the paper, and I am sure if it could have been heard in its entirety there would be very little to add. I believe that the condition of the myocardium—excluding certain complications, such as adherent pericarditis—largely influences the prognosis.

Dr. Jacobi: I recently saw a case of recovery from heart disease which struck me as remarkable. The patient was a child, seven years old, who was brought to the hospital suffering from rheumatism of the left shoulder. In addition there was an old and very hard double mitral murmur, systolic and diastolic, and a moderate amount of dilatation and hypertrophy. Within a day of his admission the patient had a fresh attack of endocarditis. The murmurs became softer and longer. Two days later a pericarditis developed, with effusion and dulness over the left side of the chest. Then she had an attack of double pneumonia, followed by rheumatism of the right shoulder and another attack of endocarditis. After a couple of weeks the rheumatism involved the left ankle-joint, and there was a fresh attack of endocarditis. The patient spent about two and one-half months in bed, and when she left the hospital she was better than she had been for years. Instead of the two murmurs which she had when she entered the hospital she now has only one, a soft systolic murmur.

The improvement in the condition of the heart in this case is probably due to the fact that during the fresh attack of endocarditis, both new and old, old exudates would soften and melt. Thus the new inflammation of the endocardium would act beneficially upon the results of the previous one. Thirty years ago I remember that oculists would occasionally treat an old pannus by inoculating the diseased eye with the secretion of gonorrhœa, thereby lighting up a new inflammation, and as a result the pannus would not infrequently disappear. Probably this practice has since been abandoned.

Dr. Frederick I. Knight: This subject is a very important one. We have all learned to be very careful about the prognosis of valvular disease following rheumatism in childhood. I remember one case, a young man who is now about twenty-five yars old, whom I first saw at the age of twelve years. He was then suffering from serious valvular disease—aortic regurgitation—following an attack of rheumatism. He spent that summer on a yacht, and was not allowed to make any exertion on account of the violent action of the heart. He gradually recovered, and I did not see him again for several years, when I was surprised to find him a strong, robust college boy. His object in coming to see me was to ask whether it would be advisable for him to take part in a rowing race. I found the heart very large, with perfect compensation, and still signs of regurgitation. I told him that I though it would be unwise for him to take part in the race, but he did so in spite of my advice, and he did not seem to suffer seriously after his two-mile row. Subsequently he gave up severe exercise. I occasionally see him now in a social way, and he is apparently enjoying excellent health.

TREATMENT OF CARDIAC ASTHENIA OF PNEUMONIA.

By HENRY L. ELSNER, M.D.,
SYRACUSE.

The treatment of the so-called cardiac asthenia of pneumonia has been exceedingly discouraging to the profession. The sudden and overwhelming toxæmia has often staggered the therapeutist because of his weakness to cope with it. A persistence in the methods of treatment which have become time-honored and routine has done much to rob the patient of the resistance which is absolutely needed to combat successfully a poison at once malignant and destructive.

To prevent the onset of circulatory embarrassment, and to relieve this condition when present in acute pneumonia, requires the breaking away from the empirical practices of the past and the acceptance, as indications for treatment, of the conditions which clinical and bacteriological experiences have made positive after faithful observation and experiment.

I fear that we have too long limited ourselves to the consideration of the heart alone when administering to our pneumonic patients, disregarding entirely the condition of the peripheral vessels, particularly the arteries.

Riegel[1] long since demonstrated by sphygmographic tracings that most infections are associated with reduced arterial tension. Rosenbach[2] emphasizes the importance of the vasomotors in experimentally induced infectious endocarditis, and Buchard,[3] at the Tenth International Congress, dilated on the baneful influences of bacterial poisoning on the vasodilating centres. It remained, however, for Romberg[4] to

make a series of experiments and to associate these with clinical data. These have done much to place the treatment of the circulatory failure in pneumonia upon a scientific basis.

In the *Berliner klinische Wochenschrift* he reports a series of convincing experiments with the bacillus pyocyaneus and the pneumococci of Fränkel, made for the purpose of determining the relative importance of the heart and the vasomotor system in the production of the circulatory failure in these infections. He concluded that the circulatory symptoms which we have considered to be due entirely to heart weakness, are, in truth, intimately associated with demonstrable change in the vasomotors.

A literal translation of his concluding sentence would read as follows: "We must consider with the heart weakness the weakness of the vasomotors, and with the cardiac paralysis that of the vasomotors also." Later, in 1896, before the Fourteenth Congress for Internal Medicine, and at the meeting of the German Naturalists and Physicians in Lubeck, Passler and Romberg[5] reported a series of experiments which serve to prove the relative importance in the production of the so-called heart weakness in pneumonia, of the heart itself, and of the vasomotor system.

Animals were inoculated with the pneumococci and the change in arterial tension carefully measured. To give the many experiments in detail would be tedious, but these were decisive. The experiments made with the pneumococci and the bacillus pyocyaneus, in which rabbits were inoculated, proved conclusively that the vasomotors were weakened and showed evidences of paralysis. These experiments were carried to a logical conclusion after many days of painstaking work, and as the result we are to-day in a position to teach that the toxins exert their baneful influences upon the vasomotor centres in the cord.

These observers, in the report just mentioned, speak of the behavior of the heart as branding the infections experimentally produced. In pneumonia, for a time, the arterial tension is kept up to the necessary height by what they call an

excited heart action. Infection with the pyocyaneus bacillus proved that the heart action was seriously retarded. Both these observers and all who worked with them, including Bruhns, agree that the circulatory disturbances found in the vasomotor system were due to the effect of the toxins on the vasomotor centres in the cord, and were of far greater import than was the condition of the heart.

In this country Van Santvoord[6] has done excellent work in this same field. Sphygmographic tracings were taken in eighteen cases of pneumonia, and these seemed to confirm the conclusions of the German observers. While the experiments to which I have thus fully referred establish the great importance of the vasomotor system, more particularly the vasomotor centres in the cord in pneumonic toxæmia, the practical clinician will always, in formulating a rational treatment of the changed circulatory conditions, keep before his mental vision the association of an enfeebled heart with dilated peripheral vessels.

Primarily we are dealing with a toxæmia. This leads to the cardiac asthenia in which we are likely to have not only marked changes in the right half of the heart, but far-reaching degenerative changes in the muscle, heart clots, and vasomotor paralysis. To this must be added the obstruction in the pulmonary circuit. Fortunately the toxæmia is short-lived, but its effect on heart and vessels is malignant, and when these suffer the treatment must be radical, prompt, and sustaining.

I must still further preface what I shall have to say on the subject of treatment, by remonstrating with all my force against the indiscriminate use of such remedies as lower the vitality of these patients while they reduce temperature. All antipyretics, except cold, which suddenly depress temperature do so at the expense of vital force, and are apt to rob the patient of needed resistance, or they may cause fatal and sudden collapse. Give nothing which at any time in the course of the disease acts as a cardiac depressant. Let your treatment from the beginning be tonic.

I quite agree with von Jaksch[7] when he says "the coal-

tar antipyretic from antipyrin to lactophenin have their value, but not as antipyretics in the true sense, but as nervines." He says "these are indispensable." This latter statement the experience of the American physician contradicts absolutely. The indiscriminate use of nitroglycerin is a growing evil with the profession of this country. If we have in pneumonia lowered arterial tension from paralysis of the vasomotors, why give a drug which increases that condition? No one denies the paralyzing effect of this drug on the vasomotor system.

It has been demonstrated also that the vagus is paralyzed; thus inhibition is removed from the heart by large doses of the drug, and it is assumed still further by Brunton that the blood loses its power of absorbing and conveying oxygen. These are the conditions which we must seek to prevent.

The writer has frequently seen patients who have been treated with some one of the numerous cardiac tonic tablets which we now find in profusion on the market—all of them containing nitroglycerin with digitalis and a variety of other so-called heart tonics—in which the patients have had the most alarming cardiac and respiratory insufficiencies, with rapidly lowering arterial tension, and in which the firm believers in nitroglycerin have, with renewed energy, given increasing doses of the drug, only to aggravate the symptoms and produce paralysis.

Nitroglycerin may have its uses in overcoming peripheral obstruction where the arteries are tense, sclerotic, or narrowed, against which the heart is laboring. This condition occasionally presents with pneumonia in elderly subjects, and may be associated with interstitial nephritis. My experience with these cases has been very unfortunate; with slight change in arterial pressure reduced by the drug and a slow pulse I have been unable to control the heart; tachycardia finally followed, and the patient died.

But in pneumonia where we already have paralysis of the vasomotors, it would seem, from the experience of the past, that nitroglycerin is a dangerous drug, in spite of the fact

that one or two authorities of note have recommended it in this condition to relieve the overtaxed and dilated right heart —a condition which it would be far better to relieve by the treatment which I shall suggest, or by venesection in rare and selected cases, than by the use of a drug which causes still further paralysis.

I have looked into this subject very carefully, and fail to find any authority founded upon physiological experiment which justifies the use of nitroglycerin as it is to-day used by many without an appreciation of one or more of the factors which underlie and aggravate the obstructed circulation.

Van Santvoord[8] in his paper pictures a series of convincing tracings taken from a patient during the acute stage of the disease—"one taken during convalescence and one a few moments after the latter, when the circulation was under the influence of a very large dose of nitroglycerin." It is difficult to differentiate the first from the last, so similar are they.

A recent experience with a young physician would have been amusing had it not been pathetic at the same time. He had been taught to bleed his patients "into their own veins" by the use of veratrum viride. The patient was a man whom I had treated for myocardial degeneration, and who fell into the hands of the young practitioner, suffering from acute pneumonia.

I saw him on the seventh day. The physician expressed himself as anxious to have me see his patient, because he wished to be assured that his treatment was all that could be desired. For seven days this unfortunate victim had received one drop of the strong tincture every hour, and when I saw him in extremis, blue from carbonic-acid poisoning, he was still receiving what the young doctor had been taught to consider his concealed lancet.

How many physicians have been misguided and how many lives have been lost from the persistent use of this cardiac depressant it would be difficult to compute. I never saw a case of pneumonia recover which had been rigorously treated with this drug.

The secret of sueeess must be found in the use of sueh methods as will restore or sustain cardiac strength and arterial tone. There are two drugs which, when properly administered, meet the indications presented by the eirculatory changes of pneumonia due to paralysis of the vasomotor centres and the dilated condition of the arteries. These are strychnine and digitalis. This statement is not new to the profession, and I will occupy no time in emphasizing it.

I wish, however, to insist upon the administration of these drugs hypodermatically. The results are more certain and more prompt. Strychnine may be administered in comparatively large doses while the patient is being watched by skilled attendants. Digitaline may take the place of digitalis. With the large doses of digitalis as recommended by Petresco[9] I have had no experience. How are we to treat the resulting cardiac insufficiency? By sustaining the heart and vessels without causing unnecessary wear or tear during these hours of imminent danger. To bring to your notice my method of aecomplishing this end is the objeet of the paper.

The profession has long since learned the value of the diffusible stimulants. Unfortunately their effeet is evanescent; hence my plan of treatment ineludes the administration of these at very short intervals during the continuance of cardiac asthenia. The stimulating effect must be eontinuous during the critieal period. The remedy must be renewed before the preceding dose has lost its effect. In no other way can we accomplish the desired result.

During the past I have treated this eondition by administering, every fifteen minutes, fifteen drops eaeh of the eompound spirits of ether, aromatic spirits of ammonia, the eompound spirits of lavender, and the tincture of valerian. This is kept up day and night until the pulse shows improved tone and the heart aetion is decidedly better.

The valerian is added beeause of its quieting effeet when administered in these small doses with the diffusible stimulants. Oeeasionally we meet patients who have been unable to retain the mixture when it contained valerian, hence we

have been forced to omit it and to substitute either pure whiskey or brandy in corresponding doses. If the Hoffman's anodyne is distasteful it has been omitted, and the dose of the ammonia and lavender has been doubled. The frequent administration of the compound has not seemed to annoy the patients, for, as a rule, they are not awake longer than is necessary to swallow the remedy. I have still further insisted upon the internal administration every two, three, or four hours, according to the urgency of the symptoms, of 0.015 gramme (one-quarter grain) doses of spartein sulphate, with from 0.24 to 0.36 (four to six grains) of caffeine. Both of these drugs must be administered in larger doses than are usually prescribed if desirable results are to be obtained. They affect favorably the weakness of the right heart.

Finally, the alcoholic stimulant upon which I depend is Tokay wine. I know of no stimulant with which I have accomplished so much, and which, in my hands, has saved so many patients as this Hungarian wine. This is administered in tablespoonful doses every half-hour, and is given with the ethereal stimulant when due. Occasionally it is necessary to use high rectal injections of coffee and whiskey and hypodermatic injections of ether and oil during periods of collapse. The administration of the diffusible stimulants as above recommended, with the numerous other remedies suggested, appears to you, I can readily understand, as bordering upon polypharmacy. To this charge I plead guilty, but in extenuation would say that we are dealing with a condition of extreme danger—one which must be met by rigorous and continuous medication. The remedies are not antagonistic.

Because of these facts we must be ready to supply the remedy anew before the heart and arteries have again lapsed into a condition which, if allowed to continue, will favor the formation of heart-clot, pulmonary œdema, and carbonic-acid poisoning.

In connection with this subject I hardly know what to say of the use of oxygen. In hospital and in private practice we have learned to use it in a sort of routine fashion. Whether

it is really productive of great good remains an unsettled question.

There is an unfortunate class of cases in which we find marked heart weakness very early, with pulmonary œdema and a very profound congestion which seems to flood the lungs with a bloody serum. These patients often die during the first thirty-six hours, in spite of any known treatment. The treatment to which I have referred cannot be carried out without skilled and faithful nurses. My experience with this method has been satisfactory to me and gratifying to my colleagues on the hospital staff as well as in consultation practice. Patients have been saved in whom the condition hardly appeared to justify the great labor of carrying out the treatment.

References.

1. Riegel. Volkmann's klinische Vortrage, Nos. 144, 145. Uber die Bedeutung der Pulsuntersuchung.
2. Rosenbach. Arch. für. Exp. Path., Band ix.
3. Buchard. Tenth International Congress, vol. i. p. 59.
4. Romberg. Berl. klin. Woch., 1895, Nos. 51, 52.
5. Passler and Romberg. Berl. klin. Woch., April 27, 1896, p. 382, Fourteenth Congress Internal Medicine.
6. Van Santvoord. New York Medical Journal, 1898, lxviii. pp. 521–525.
7. Von Jaksch. Berliner klinische Wochenschrift, 1896, p. 363.
8. Van Santvood. Loc. cit., p. 525.
9. Petresco. Therapeutische Monatshefte, February, 1891.

DISCUSSION.

Dr. Walker: I would like to ask Dr. Elsner whether he condemns nitroglycerin in all cases of pneumonia, or only in those cases where there is cardiac asthenia.

Dr. Elsner: Nitroglycerin, I believe, is contraindicated unless pneumonia is associated with diseased arteries causing increased tension, unless there is some obstruction in the peripheral circulation which the heart is laboring to overcome. Occasionally, in old patients, we find interstitial nephritis and arterio-sclerosis associated with pneumonia; in such cases I have used nitroglycerin. I must confess that in these the prognosis has been bad, as I have pointed out elsewhere. Pneumonics with narrowed arteries, the result of syphilitic endarteritis, might be given nitroglycerin.

Dr. Walker: I have had such satisfactory results with nitro-

glycerin in pneumonia, first using it at the suggestion of Dr. DaCosta, that I wish to say a word in its defence. In my experience it has benefited these patients more than all the ammoniacal stimulants. I remember a very severe case of pneumonia which I saw about two years ago. The patient was a boy. I gave him one-hundredth of a grain of nitroglycerin every two hours for at least two weeks, with nothing but beneficial results. I recall another case, a lady who came to Philadelphia from Winchester. During her visit she developed pneumonia, and her physician said she could not possibly recover. Under the use of nitroglycerin, however, the heart improved and the patient made a good recovery.

I wish to mention another therapeutic agent which I have found of great service in cardiac asthenia, and that is dry cupping. My patients are dry-cupped as long as the engorgement persists and whenever new pains occur.

Dr. Curtin: Nitroglycerin loses its effect in about three hours. In order to have a continuous effect the dose should be repeated every two or three hours.

Dr. Judson Daland: I recently had an opportunity to make some experiments with digitaline on two men in normal health, with the exception of a slight bronchitis. I increased the dose of digitalin (Merck's German) until the patient was taking one-quarter of a grain every four hours, and the drug produced almost no effect.

Before we depend on digitalin in these cardiac cases we should be in possession of more information. In certain cases where heart weakness shows itself, and dilatation of the right heart is present, venesection is the only thing to be employed, and occasionally, as a preliminary thereto, wet cupping. In these desperate cases I resort at once to the removal of blood.

Dr. Babcock: I have no doubt that theoretically Dr. Elsner's treatment is successful, and he has accomplished what he wished to; but we have all seen cases of pneumonia with paresis of the capillaries, in which nitroglycerin has been of the greatest benefit. It depends on the way in which it is used. As pointed out in the admirable paper of Dr. H. C. Wood about a year ago, nitroglycerin in doses of one one-hundredth of a grain, placed on the tongue, is a very valuable heart stimulant, whereas larger doses will do harm. In large doses it is a cardiac paralyzant, while in small doses it indirectly stimulates the heart's action. In these cases of capillary paresis we have all seen the benefit of digitalis, strychnine, and nitroglycerin, but not until the latter has been added.

Dr. Jacobi: Digitaline, which I first tried many years ago, I regard as a useless product. It is not an alkaloid, but a resinoid, and is a mixture of several things. That is why Garnier-Lamoureux's and Chanteaud's and Merck's digitaline and the various American

digitalines are different articles. I have seen Merek's digitaline given in one-grain doses without producing any effect whatever. To inject one one-hundredth of a grain subcutaneously and expect any effect from it is a mistake. The effective agent of digitalis is digitoxin, not digitaline.

Dr. Elsner: I expected, in presenting this paper, to meet the adherents of the nitroglycerin treatment. I dare say that in the majority of the cases which were cited the nitroglycerin was given in connection with other remedies which we know to be effective heart stimulants, among these strychnine and digitalis.

With regard to the substitution of digitaline for digitalis, we have occasionally done this in the hospital, using it hypodermatically. In these cases this was combined with the treatment outlined in the paper. I never in any case depend on digitalis or its derivatives alone. The effect of sparteine is certainly lasting, comparatively speaking, according to my own and the experience of most men who have used it. Its effect on the right heart makes it particularly valuable.

With regard to the use of cups, I would say that the profession does not believe that cupping has any influence on croupous pneumonia; it relieves the pain, but it certainly does not in any way influence the toxæmia. I regard venesection as a valuable measure to relieve the right heart and help the peripheral circulation. I should certainly prefer it to nitroglycerin. I am heartily in favor of the treatment outlined in the paper, for it has given me better results than I had before it was adopted.

REMARKS BASED UPON A FURTHER EXPERIENCE WITH CALOMEL IN DIPHTHERIA.

By L. D. JUDD, M.D.,
PHILADELPHIA.

Two years ago at Washington, D. C., I read a paper before this Society, published in the TRANSACTIONS of 1897, on "Calomel as a Curative Agent in Diphtheria." It was based upon my experience with this drug, covering a period of eighteen years.

In that paper I cited an experience with a case of inoculated malignant diphtheria, in a woman, fifty-five years of age, weighing two hundred pounds—the most pronouncedly malignant of any I have ever seen—due to a scratch on the finger by a child dying of this disease. That case was saved by the heroic use of calomel. Three hundred and sixty-five grains were given in thirty-five hours—twenty grains in the first dose and ten grains every hour thereafter until the characteristic action was secured. Rapid and complete recovery ensued without the slightest mal-effect due to this drug. Also in that of a child, aged eighteen months, where I gave eighty-five grains—ten grains in the first dose and five grains thereafter hourly for sixteen hours—with similar results. Independent of these I had employed this drug in twenty-three cases of the most malignant type; forty-two pronouncedly severe, and in a much larger number which might be called mild, though severe, cases. The dose varied in accordance with the severity of each.

I have had no occasion to employ a course so heroic as in the first two named, although I have treated many malignant

cases less in degree, tempering the use of the drug to meet the requirements of each. I now believe that smaller doses of calomel, oft repeated, will exert as specific an action in the majority of cases as the larger doses related in my former paper on this subject. Nevertheless I would feel perfectly justified in resorting to the more heroic treatment should the case seem to be approaching the moribund condition, and I would have no fear of any direful results from the drug.

Cases of diphtheria *due to inoculation* are comparatively rare. In twenty years I have had but three. Two of these occurred within a month after our meeting in Washington, and my experience since with those and others has suggested these further remarks. I was called to see a girl, aged fourteen years, suffering with a severe form of diphtheria, which responded readily to one-sixth of a grain of calomel given every half hour for twenty-four consecutive hours, decreasing the dose and lengthening the time of administration as she improved, using a solution of peroxide of hydrogen for frequent spraying of the throat. After securing the characteristic dejection I placed her on iron and chlorate of potash mixture. She made a rapid recovery. Two weeks later I was called to the younger sister, aged ten years, and a brother, aged six years, both exhibiting a malignant form of diphtheria, as evinced by the swollen and deeply injected fauces, engorgement of not only the post-cervical, but the submaxillary and sublingual glands. Every precaution had been taken to prevent communication with the elder sister and contamination of the house. On the third day the mystery was solved. The elder sister had been using chewing-gum when her throat was sore, and probably after the patches had fully developed. As children often do, she hid it by sticking it under the chair seats. Her sister and brother found the "bonanza" and duly revelled in their discovery. In this manner they were thoroughly inoculated. I commenced by administering to each a five-grain dose of calomel, followed every twenty to thirty minutes with one-half grain; also dissolving calomel triturates in warm water, and thus, in

suspension, securing the topical action of the drug in spraying the nostrils. I decreased the dose as improvement manifested itself. The nursing was faithfully done. Inside of four days they were safe. The one-half-grain dose was given on an average of every half hour for thirty-four doses, when I dropped to the one-sixth of a grain for twenty-four hours, and to still smaller doses until I felt that the disease was conquered. They both made good recoveries, and neither exhibited the slightest mal-effect from the calomel. It is a noticeable fact that the patient becomes stronger during the administration of this drug. There is less of local paralysis observable, and the so-called heart-failure from toxæmia is not liable to occur.

I have had a fair share of diphtheria in my practice since my last paper was presented in 1897, and not one has perished. Invariably calomel has been my mainstay. Again I state that I have never seen a case of salivation or anæmia, or of any mal-effect, that could be traceable to this drug in the treatment of diphtheria in the manner employed. *Faithful and intelligent nursing is of course required.* There is no remedy that, to me, seems so safe, and none whereby you can as securely "feel your way." By it the nature and the severity of the case may be early proven. Should it be only an aggravated attack of tonsillitis the one-eighth to one-tenth of a grain, given every hour, with the use of peroxide of hydrogen for spraying the throat, will bring a comparatively early response. Should it be true diphtheria so small a dose will bring no characteristic action in even twenty, thirty, or more hourly doses. In such courage is necessary to push the treatment by increasing the dose and, if necessary, decreasing the interval between doses until the characteristic dejection occurs. Even before this is accomplished improvement in the patient's condition will be noticeable.

By the *characteristic dejection* I mean the movements from the bowels that have the appearance of "frog spittle" and copious, like that of the green polyps observable in an old water-trough; slight greenish movements may occur before

the characteristic dejection is secured, and although improvement becomes manifest with these, a copious "frog spittle" stool is necessary to prove that the limit is reached. I am well aware of the great claims made by many in behalf of antitoxin as a curative agent in diphtheria. The consensus of opinion is far from being emphatic in approval of its use when the advanced stage of the disease is presented. That it has proved a success in the doubtful, mild, and early stages of the disease, I cannot question.

But in the light of my experience of twenty years with the calomel treatment in this disease, with the percentage of loss so insignificant, in my own as well as in the practice of others who have tested this plan, to change it for any other would seem almost like the abandonment of a certainty for an uncertainty.

I wish it to be understood that I am advocating the *fearless use of calomel only in pronounced diphtheria.* Although it is one of the most valuable of all drugs known, I realize fully the necessity of the physician exercising the greatest care and discrimination in its employment generally. That the use of calomel has been abused in times past, there is no doubt. At the same time we know that the hue and cry against it has, to the greater extent, been prompted by the so-called eclectics, who claim to have found a "vegetable calomel" in podophyllin, and who later on found their claim proven invalid; and the homœopathic sect, who have so bitterly railed against it and at the same time use it persistently, in high and low attenuations, under the name of "mercurius dulcis" ("a rose by any other name will smell as sweet"). Happily, the day of bigotry and intolerance in medicine is rapidly passing away.

DISCUSSION.

Dr. T. D. Coleman: My experience for the past nine years with the use of calomel in the treatment of diphtheria corroborates so perfectly the position taken by my friend, Dr. Judd, in his admirable

paper that I am very glad to have the opportunity of adding my testimony to what he has said. My father, Dr. John S. Coleman, toward the latter years of his life used the mercury treatment for diphtheria with such signal success that I could not fail to see the virtue of it.

In a paper which appeared in the *Journal of the American Medical Association*, February 23, 1899, he records some experiences in the use of bichloride of mercury in this disorder. In one case he gave a child, sixteen months old, nine grains of bichloride in three days, with the result that at the end of that time the child expelled a membrane containing the impression of six tracheal rings. Complete recovery followed.

Latterly he gave up the use of bichloride for the subchloride of calomel, for the reason that the bichloride tended to irritate the stomach. In one case which I now recall he gave the five-year-old son of a physician 360 grains of calomel in three days, complete recovery following. The boy is now a robust lad of fifteen years. In all his experience with mercury in the treatment of diphtheria he lost only one case, and this case he was called to only after other remedies had failed and only twelve hours before the child died.

My own experience in the treatment of the disease has been similar and equally gratifying. In an experience extending over nine years I have only one death to record where this treatment was employed, and this case was most malignant from the start, the membrane involving the nose, pharynx, and trachea.

My rule in these cases is to give a large dose of calomel at the beginning, regulating it according to the age, and then to give hourly doses until the characteristic "chop-spinach" movements appear, then the interval is increased until the drug is left off entirely.

I cannot add anything to Dr. Judd's directions for administering the drug, as I consider that they are sufficiently full to guide one. With reference to certain indefinable fears of ill effects from the drug when given in such heroic doses, I can only say that the ability of patients to take such large doses without ill effect is, in my opinion, due to the fact that the ordinary physiological effect of the drug is overcome by the poison or toxin of the disease.

I wish to say, in conclusion, that I do not believe it produces anæmia, because I believe that the disease itself does this profoundly, and any drug which so certainly cuts the disease short would by that much prevent those blood changes which are responsible for the anæmia. Furthermore, I have never seen any disagreeable effect whatever resulting from the administration of the drug, and I have gotten such universally good results from its use that I have been unwilling to relinquish it for newer and more sensational remedies.

DR. THOMAS DARLINGTON, JR.: For several years past I have been in the habit of using calomel in the treatment of diphtheria —at first merely to open the bowels in the beginning, and frequently used it every other day in small doses—one-tenth grain—every half hour until ten doses had been given.

Later I used it for inhalation by sublimation. This, however, I had discarded, and was merely using it as a laxative throughout the disease until about two years ago, when I read your valuable paper on the subject. Since then I have used it in all cases, only more courageously. I have not, however, found it necessary to give such large doses to infants, but use about one or two grains daily, divided into tenths. In most of my cases this, combined with a proper diet and a nasal douche, is all of the treatment, and I cannot at this writing remember any deaths treated in this manner.

There have been other cases that I have treated with antitoxin, some in combination with the calomel and others alone; but my experience is that many children do just as well and often better without antitoxin, and I must confess that my leaning is toward the calomel treatment.

CARDIO-ŒSOPHAGEAL GUSH AND CLICK.

By ROLAND G. CURTIN, M.D.,
PHILADELPHIA.

In studying the sounds heard in and around the heart I have observed two, a description of which I have not been able to find in the literature coming under my eye. I have every reason to think that they are extra-pericardial, and, furthermore, I am quite sure that they are œsophageal.

In this paper you will be introduced to these two sounds, both of which are synchronous with the action of the heart; and from their many points of similarity I think they may be studied together. I will call them the cardio-œsophageal gush and click. A study of the histories of the two classes, with their two sounds, will be the best way to introduce the subject.

In 1894 a young girl, aged nineteen years, called at my office, saying that she had a queer noise which a doctor had told her was a heart murmur that he could hear without applying the ear to the chest. She informed me that it was not so loud that day, for she could not hear it. I listened to her chest and found a mild, double, mitral murmur, with evidence of some hypertrophy. She then complained of dyspeptic symptoms only.

I told her to return to me if the sound came back, which she did next day. I could hear a sound with the action of the heart, which was then excited; and on withdrawing my ear one yard away from the chest I could hear it, when her mouth was open, before or early in the systole. When she closed her mouth it was very perceptibly deadened. It continued when her breath was held. On applying the ear

to the chest the sound was subdued, being less distinct than when the ear was held in front of the open mouth. It seemed like a short gush of air from the throat, modified by the upper air-passages, giving it a low-pitched, grunting quality. I could hear nothing at the epigastrium, and there was no apparent effort of the diaphragm or abdominal walls, as if expelling flatus from the stomach. The sound continued while she was talking and breathing. It did not appaer to be a moan or grunt, or like a regurgitation of air from the stomach.

I gave her some potassium bromide, tincture of nux vomica and compound tincture of cardamon and mint water. She called the next day, and I found that the heart was not beating so rapidly and that the gushing sound was absent. She said that after the first dose of medicine the heart had quieted down and the sound promptly disappeared.

A second case was that of a man, aged about sixty-two years, with dilated left and right ventricles. The sounds heard were similar to those heard in the case first reported, and could be easily distinguished across a room. The sound had all the characteristics given in Case I. A short time later the patient died suddenly. No post-mortem examination was held.

Dr. T. Chalmers Fulton, of Philadelphia, told me that he had a case which I firmly believe to have been like the two that I have just reported—of the loud, œsophageal sound. I was to have seen this case with the doctor, but the patient was absent from home the day we called. A short time afterward he was taken sick and died. The history is as follows:

He was an old soldier. For years he had a continuous grunting nasal sound, synchronous with the heart's action, and audible a great distance from the body. Dr. Fulton said that on a still day he had heard it across the street. It was much louder when the mouth was opened, and after taking a drink, or when excited. In fact, anything that increased the action of the heart seemed to increase the sound.

I examined the heart, which Dr. Fulton removed after death, and found nothing except that it was generally hypertrophied. Two skilled pathologists examined it and informed me that there was nothing in the arrangement or construction of the papillary muscles, tendons, valves, or any other part of the heart to account for any abnormal sounds. From the history and post-mortem I feel satisfied that this case was one similar to the two before reported.

In this class of cases the sound was diagnosticated in two instances as being of endocardial origin. It is true that they all had organic heart disease, causing enlargement of the organ. The sound heard so plainly was evidently exocardial, and seemed to be from the œsophagus, or, at any rate, from the fauces. I cannot understand how the heart could be powerful enough to force air out of the lungs with sufficient rapidity to cause so loud a sound, especially during inspiration. It has occurred to me that some of the cases which have been reported as having had extraordinary loud murmurs during life, without pathological changes being found after death, might have been of the kind here reported. Two were so considered by the medical attendants.

The sounds always suggested the expulsion of a small quantity of air. My reasons for believing that it is an œsophageal sound are:

1. It seems to come from the fauces.
2. Closing the mouth muffled the sound.
3. On auscultation it was heard down to the middle of the sternum, and not below that point.

The only explanation that occurs to me which seems at all plausible is that the heart in diastole pressed out of the œsophagus a small amount of air, which was readmitted during the contraction or systole of the organ. At one time I thought it might be that the contraction of the heart opened the gullet, so as to allow the escape of flatus from the stomach. But this thought was dispelled when I considered that the stomach could not possibly furnish gas enough to continue the sound for days or, as in one case, for years.

Œsophageal Clicking. This is a short, sharp, clicking sound, heard, generally, at the end of expiration. It is plainly audible as coming from the fauces, and is quite evanescent, being clearly distinguishable to-day and gone to-morrow. It is absent before a meal and present during digestion. This sound has all the peculiarities of modficaton of the louder, gushing sound, being dulled by closing the mouth. I have noticed this clicking in several cases.

One case in my own family I have studied very carefully and for a number of years. When he was twenty-five years of age this clicking was often noticed with the action of the heart toward the end of expiration, when one, two, or three small, sharp clicks were heard, stopping as soon as the next inspiration began. This sound was often noticed after a full meal, drinking of stimulants, active exercise, and especially after riding rapidly on the back of a rough trotting horse. It was sometimes noticed, without any assignable cause, after lying down at night. The sound was subdued by closing the mouth, and generally stopped by curtailing the respiration. A forced, held expiration increased the number of clicks. In this case there was no evidence of hypertrophy of the heart or any endocardial disease.

To my personal knowledge the case had this sign, more or less, for thirty-eight years, being absent for years at a time. This is one of the three or four cases that I have seen. In all of them the clicks were interrupted by inspiration.

This sound is so slight that it might be present and not be noticed by the physician or the patient. Therefore, I suppose that it is more common than the louder sound, as I have never had a patient sent to me in whom it was previously detected by a physician. My explanation of this click is that it is caused by the separation of two moist surfaces. That the heart causes it is proven by the fact that it keeps time with the heart's action. I found in the case reported that it occurred about the time of the systole of that organ. In this connection it may be of interest to state that Dr. Walsh, of New York, has called my attention to the fact

that Bernhardt, of Berlin, has reported cases of clonic contraction of muscles, in the neighborhood of the Eustachian tubes, causing a sound or click in the ear. These sounds could be heard at a distance from the ear, and occurred as often as eighty to one hundred and twenty times a minute, without reference to the heart's action.

There is another faucial clicking sound which is quite common. It occurs with each sound of respiration, and is frequently found in cases with old pulmonary cavities, especially near the apex of the lungs.

This also comes from the fauces, but can be easily differentiated from the click here described by observing that it occurs with each respiration, and is not synchronous with the heart beat.

I am inclined to think that these two unusual sounds that I have described are produced by the action of the heart on the œsophagus, so I have presented them together in this paper.

Let us look at the anatomy of the parts :

The œsophagus, as found in the post-mortem room, is contracted and flattened. The walls are flaccid, having no elasticity. The position of the œsophagus is found to be directly behind the heart, with a small intervening space between it and the left auricle and left ventricle. They are so close that the heart has been wounded by foreign bodies which first became lodged in the œsophagus. The aorta is found to the side, and not far away. Anatomists inform us that Trutz has found, between the pericardium and the anterior surface of the œsophagus, a muscular slip or band. It occurred to me that if this muscular slip were abnormally developed it might influence the œsophagus, provided the pericardium were adherent. However, the study of the anatomy of the parts does not throw much light on the subject under consideration. If the œsophagus were an elastic tube the explanation would seem to be easy. A mediastinal tumor, with a rigid œsophagus, might introduce conditions that would assist us in our explanation of the loud sound which we are consid-

ering. You will remember that in all the three cases in which the loud sound was heard the heart was enlarged.

Recapitulation. 1. The sounds were heard at the beginning of the systole of the heart, and in two cases diagnosticated as endocardial murmur.

2. The sounds of both were modified by respiration, being heard most markedly at the end of expiration.

3. They were all more or less evanescent, except in the case of the old soldier.

4. They were increased or developed by a full stomach, stimulants, or excitement.

5. The sounds were increased by opening the mouth and dulled by closing it.

6. They were heard by auscultation at the middle of the sternum, but very much subdued as compared with the sound heard at the open mouth.

7. The sound was not transmitted in any direction from mid-sternum.

8. They were not heard by auscultation over the stomach.

My object in presenting these cases is to gather observations and opinions from the members of this Association.

DISCUSSION.

Dr. Frederick I. Knight: Dr. Curtin's paper recalls to my mind a patient, shown some years ago by Dr. J. Solis-Cohen, upon whom he had performed laryngectomy for the relief of malignant disease. The entire larynx had been extirpated and the trachea stitched to the throat in front, so that there was no possibility of air getting through the mouth from the lungs. In spite of this the patient could speak in an audible voice. He could be heard for some distance, and the question arose, Where did he get the air necessary for articulation? The only conclusion we could reach was that the air came from some pharyngeal pouch or from the œsophagus.

THE BEARING OF EMBOLISM ON THE COURSE OF VALVULAR HEART DISEASE.[1]

By WILLIAM M. GIBSON, M.D.,
UTICA, N. Y.

In giving the prognosis in valvular lesions of the heart or in aiding the patient to plan for his future there is probably no more disturbing element to be considered than the occurrence of an embolism. If in these cases there exists proper compensation for the obstructing lesion or insufficient valve we generally expect a reasonably good length of time to elapse before the compensation begins to fail; provided, of course, no reinfection of the lesion occurs and the patient is not subjected to mental or physical strain, or weakened by illness; and when the compensation begins to fail due warning is generally manifested either in dyspnœa or interference with digestive or renal functions, or by the commencement of œdema in some part of the body.

Gibson, in his *Diseases of the Heart and Aorta*, in considering the prognosis and treatment of chronic affections of the heart, writes as follows: "From the length of time which has elapsed since the commencement of any valvular lesion it is possible to predicate to some extent what will be the probable future of the lesion, for in cases which have existed for a considerable time without serious inconvenience there will naturally be a hopeful expectation. When a tendency to dilatation disturbs the compensatory process of hypertrophy the prognosis is serious, and any tendency to cardiac failure or systole is of evil omen. . . . The effects of the lesion upon the system elsewhere must be judged by the condition

[1] Read by title.

of the digestion, of the blood, of the lungs, of the kidneys, and of the brain. . . . When attempting to make a forecast of the future and to lay down rules for the guidance of any case of valvular disease the age and sex of the patient will necessarily set natural limits to the possibilities of management; it is clear that the conditions are subject to entirely different agencies when the valvular disease occurs in early, middle, or later life, and in almost the same way the sex of the patient exercises a preponderating influence."

It is evident that these conclusions are based on a study of the part played in valvular heart disease by compensatory hypertrophy. An embolism, however, often occurs when compensation is at its best and when the person is not prevented from attending to the matters of every-day life.

Gerhardt's advice, not to over-seriously consider a murmur which exists in a heart not increased to the right and in which the apex beat is not out of normal position, or the second pulmonary sound is not accentuated, is probably a suggestion we all accept; but a few vegetations on the valves of the heart may give rise to a murmur and yet offer no great obstruction to the blood current. So slight an obstruction will hardly call for increase of heart muscle enough to displace the apex or extend cardiac dulness to the right. The detachment of one of these vegetations may produce fatal cerebral embolism or give rise to serious disturbance of digestion or kidney function, or produce gangrene in an extremity. The age, sex, and environment, which, as Gibson says, necessarily set limits to the management of valvular heart disease, give us no intimation of the danger of the occurrence of an embolism.

Emboli swept into the circulation arise chiefly from vegetations formed on the valves of the heart, from atheromatous changes in the aortic valve, and also from coagula formed in the heart cavities and pulmonary veins. Thrombi in the veins of the lower extremities furnish embolic masses which are frequently the cause of hemorrhagic infarction of the lungs. The consequences of an embolism depend very much

on the nature of the embolus, the area of the bloodvessels occluded, and the function of the organ disturbed (Weichselbaum). The effect of an embolism on heart action is modified somewhat by these same considerations; but the nature of the infarction, whether it is anæmic, hemorrhagic, or septic, must also be taken into consideration. Recently formed coagula or vegetations causing infarction will be absorbed more quickly than older ones; and probably emboli thrown off from the valves soon after an acute endocarditis carry the infecting germ, although the febrile symptoms may be in abeyance. Infarctions which are anæmic will disturb compensatory heart muscle less than hemorrhagic infarctions, unless large areas or vital organs are involved.

An embolus may travel wherever the blood current offers a channel for it, but its direction seems to be special rather than general, the ramifications of the pulmonary artery being most frequently affected. The vessels of the lower lobe of the right lung are the most often occluded; the power of the blood current and the weight of the embolism are (according to Eichorst) responsible for this selection. The renal, splenic, mesenteric, and cerebral arteries and the branches of the cœliac axis appear to suffer next in frequency. The condition of the heart wall, while it may not be especially concerned in the detachment of masses from the valves, has much to do with the formation of coagula. Mitral disease, causing considerable hypertrophy and dilatation of the right cavities, is frequently complicated by pulmonary embolism.

Recurring pulmonary hemorrhages in mitral disease are generally the results of infarctions. Slight hemorrhages often relieve an overworked heart and engorged lungs, but the more profuse bleedings exercise a baneful influence on the heart muscle. The loss of blood is often difficult to make up, and the long confinement indoors necessitated deprives the patient of the much-needed open-air life. The pneumonias following infarctions also tend to destroy compensatory muscle and interfere very seriously with general nutrition.

A cerebral embolism may give rise to disturbances of brain function, ranging anywhere from slight disturbance of consciousness to a fatal apoplexy. Infarction of the renal vessels remaining purely anæmic will probably not disturb the heart's action very seriously, but suppurating areas developing afterward may give rise to very grave conditions and ultimately renew endocardial inflammation. Infarctions of the branches of the cœliac axis are frequent, but escape notice in many instances. In the mesenteric arteries they may give rise to serious peritoneal inflammations, and even gangrene of an intestine.

Infarctions of the peritoneal bloodvessels during the course of acute endocarditis, or after the subsidence of the acute symptoms, may give rise to symptoms demanding operative relief. Such complications should be carefully studied, and the assistance of an experienced surgeon will often aid in detecting septic processes which may escape the notice of the general practitioner.

A little over a year ago a young married woman was brought to one of our hospitals, apparently in a serious condition, owing to some obscure abdominal infection. Shortly before the appearance of the abdominal symptoms she had passed through a rheumatic endocarditis, rather severe in type and of considerable duration. An exploratory laparotomy failed to discover any disorder of the uterus and its appendages or the appendix. Her symptoms, however, persisted, and after a few weeks became so serious that another operation was decided upon. A distinct tumor could now be made out in the region of the right kidney. At the second operation it became necessary to remove a large amount of inflammatory tissue and adhesions which had occluded the right ureter. The kidney was found a cystic mass, which was also removed, and the woman then made a good recovery. It was, of course, impossible to detect the site of the original infection, but it seems more than probable that the infecting material had been conveyed by embolism from the endocardial inflammation.

Pepper, in his article on "Diseases of the Endocardium," in the *American Text-book of the Practice of Medicine*, calls attention to the occurrence of small infarctions in the vessels of the skin, producing petechial eruptions and also areas of subdermal effusion. A case of mitral regurgitation with good compensation, under my observation, developed two of these areas, one in the skin of the left thigh and another on the outer surface of the right leg above the ankle, which remained for weeks in a low state of nutrition bordering on gangrene. The effect on compensation was very marked, and the patient has not succeeded in regaining the loss due to this long drain on nutrition.

Gangrene of the extremities following embolism is necessarily of very grave import in valvular heart disease. The tax suddenly thrown on compensation by the occurrence of gangrene is liable to produce extreme dilatation, even if the patient rallies against the general effects of septicæmia.

During the last epidemic of influenza I saw, with Dr. A. R. Simmons, of Utica, a case of mitral stenosis of long standing that had suddenly developed gangrene of the right foot and leg. Influenza with a right lobar pneumonia had preceded the gangrene, but the compensatory hypertrophy had not suffered very much. Amputation above the knee arrested the gangrene, and the stump healed kindly. A recurrence of pneumonia, probably embolic, proved too much for the patient's strength. An examination of the amputated leg disclosed an organized clot, nearly two inches long, at the bifurcation of the popliteal artery. The probabilities are that with good care this woman would have again secured a fairly good compensation, even after the long strain due to influenza and pneumonia, had not the two infarctions occurred.

The following cases are reported as illustrative of the effects of embolism in heart lesions which were not judged to be of a serious nature :

Case I.—A gentleman, aged thirty-nine years, whose family physician had casually discovered a heart murmur when looking him over for some minor affection, came to me

for further advice. While there was an unmistakable murmur at the aortic valve, I could not make out that cardiac dulness was increased to the right or that the apex beat was out of normal position. Apparently the lesion—aortic obstruction—was of slight concern, for he never complained of dyspnœa, and the renal and digestive functions were perfectly carried on. While sitting in his office, perhaps half a year after I saw him first, he was taken suddenly with giddiness, vomiting, and partial aphasia. There were no symptoms of paralysis in the extremities, but it was fully three weeks before he recovered full use of the speech centre. Two years later he was taken in a similar manner, and suffered from converging strabismus of the left eye; there was also considerable mental depression accompanying this attack. During the next few years he had six attacks, all of which were accompanied by vomiting and some impairment of intellect. In December, 1896, while talking with his family, he was seized with a right hemiplegia and aphasia. By the end of the following March he had regained muscular control and the power of speech, and was apparently as well as ever. A month later he passed suddenly into coma, which terminated fatally in two days.

Case II.—A woman, aged thirty-six years, during her second pregnancy suffered an attack of acute rheumatism in the second month, which lasted about two weeks. She had a recurrence of the rheumatism in the fifth month, which caused a slight endocarditis, but from that time on was free from rheumatism or any febrile disturbance. She carried from a former attack in childhood a heart murmur which was heard with the first sound of the heart over the aortic valve. She had been carefully examined by several physicians, who pronounced the murmur to be of no serious consideration. The only symptom referable to a disturbed heart that she had ever complained of was a slight but persistent dyspnœa that occurred when travelling or living in elevated regions. I had examined her heart from time to time during a period of three years prior to her pregnancy, and while the

murmur was always present, no increase of cardiac dulness could be detected. There was, however, slight accentuation of the second sound. She had a normal labor and convalescence up to the eleventh day. On the evening of that day she was taken with a severe chill, which was repeated in four hours. These rigors were followed by a rise of temperature to 105° and profuse sweating. The chill recurred at noon the next day. A careful examination of the uterus and appendages was made by Dr. J. H. Glass, who pronounced them free from infection. Nothing abnormal could be discovered in the lungs. The examination of the urine, however, showed it to be highly albuminous, and numerous granular casts, studded with pus-corpuscles, were found under the microscope, but there were no evidences of cystitis present. Urinary examinations had been made several times before parturition, and also afterward, but nothing abnormal had been detected prior to the chill. Two days after the chill the patient complained of pain and soreness in the region of the left kidney. The urine became scanty and high colored and the amount of albumin increased; the pus casts became more numerous, and the patient passed into a serious condition of septicæmia, which lasted about two months, but finally made a good recovery.

I am very certain in this case that there was no external cause for the suppurative nephritis, and have always believed that it was due to an embolism of a branch of the renal artery, and that the infection retained from her previous endocarditis changed the infarction into an area of suppuration. This woman has borne three children since without a recurrence of any renal disturbance, and the condition of the heart wall remains practically unchanged in spite of the septicæmia; but in her case the question of the recurrence of an embolism may be a more serious factor in the next few years than endocardial change and slow impairment of heart muscle.

Case III.—Last fall a gentleman, aged seventy years, was thrown from his bicycle and sustained a fracture of the left humerus. He had suffered several attacks of acute rheuma-

tism, scattered over a period of thirty years, but had escaped endocardial disease until the winter previous to his injury. An attack of rheumatism in January was complicated by endocarditis, which was aggravated by another attack in March. He was soon able, however, to take his place in his bank with very little inconvenience. The month of June he passed at the seashore, where he regained his usual strength. An examination of his heart at this time disclosed slight murmurs at the aortic and mitral orifices; the cardiac dulness was not much increased, and the position of the apex beat was nearly normal. An examination again in September showed about the same state of affairs. Aside from slight dyspnœa, which came on from riding his wheel up steep grades, the man was apparently free from symptoms of cardiac disease. Immediately after his injury he was placed in bed, and after the shock consequent to the injury had passed off the fracture was reduced and bandages were applied. He remained in a very comfortable condition until the next afternoon, when he was suddenly taken with a sharp, stabbing pain under the umbilicus. The pain became more and more violent, and demanded the free use of morphine for its relief. The day following the pain was even more intense, and was not relieved by a high enema which brought away a large amount of feces. The temperature remained normal, but the pulse was hard and somewhat accelerated. Symptoms of intestinal obstruction now developed, and the condition of the patient became very grave. His wife and son and the patient himself begged for operative interference. With the hope that possibly an old hernia might in some way be the cause of the intestinal obstruction, an exploratory laparotomy was performed by Dr. Kilbourne and Dr. J. D. Jones. As the intestines were laid out on the sterilized towels a gangrenous fold of intestine rolled into view. It was found that the supplying mesenteric artery had been occluded by an embolic mass. The intestines were drained and the abdominal incision closed, as it was evident that the gangrenous portion

of the intestine was too large to permit of its removal. The patient died of exhaustion the day following.

The occurrence of embolism during the course of valvular heart disease is certainly not an infrequent complication, but prophylaxis offers little or nothing for its prevention compared with the effect of good management on compensatory hypertrophy. Recognition of even a slight embolism, however, is of great importance, for much can be done through rest and administration of proper remedies, surgical relief included, toward preventing some of its more serious results.

A CASE OF ANEURISM OF THE AORTA.

By THOMAS DARLINGTON, M.D.,
NEW YORK.

The patient was a man, aged forty-eight years. He showed a history of syphilis. Some years before, when a young man, he had been stabbed in the sternum with a shoemaker's knife. Possibly an injury to the aorta was the beginning of the aneurism. It was discovered comparatively early. It gradually became larger, and appeared on the neck above the sternum, and, absorption of the cartilage taking place in a few months, the whole sternum gradually disappeared, and in its place was the aneurism, rounding up above the surrounding parts, in which the pulsation could be distinctly felt and seen.

The front wall then became thinner and thinner, until in two places it was apparently as thin as paper. No pressure was made upon the tumor, for fear of rupturing the sac. One day, while lying on a sofa next to a wall, he coughed, and the sac ruptured externally, sending the blood up the wall nearly to the ceiling. Strange to say, there was simply one gush, and then the blood ceased; and this is the interesting part of this case. A large clot which was in the aneurism forced its way into the opening and effectually plugged it. I was sent for at the time, and found the man collapsed from loss of blood and fear. There was a little ooze, which I stopped with styptic cotton, and then photographed him.

Three days afterward, during a coughing spell, the aneurism again ruptured in another place; but this time no clot filled the opening, and the man succumbed immediately.

Aneurism of the aorta.

Unfortunately, I was unable to obtain a post-mortem.

I recite this case for these reasons:

1. Because of the interest which attaches itself to the stab wound as a possible cause of the aneurism, or was it syphilis?

2. Its large size, having caused the absorption of the whole sternum.

3. Principally because of the filling of the opening with the clot when it ruptured.

EMPYEMA FROM A SURGICAL STAND-POINT.

By JOHN C. MUNRO, M.D.,
BOSTON.

I HAVE very little that is new to add to a subject upon which so much has been written, but there are a few points that I think will bear repeated emphasis until it is more thoroughly recognized that pus in the pleural cavity needs as early and complete evacuation as it does when in the abdominal cavity. My conclusions are drawn entirely from my own experience, and they have been reached only after much thought and observation.

ANÆSTHESIA. In the hands of a skilled etherizer there are very few cases in adults, to which class my experience has been largely limited, in which ether cannot be given; but unless properly given, and with intelligent supervision of each inspiration, it may be dangerous. It can be given to children easily controlled, while in very young children the stage of unconsciousness is so rapidly attained that its use is safe in a large majority of cases. The rule of giving fresh ether or fresh air, avoiding asphyxiation, must always be observed. I have seen collapse from ether in an infant in one case, but in this instance and probably in similar cases the operation had been so long delayed that anything beyond aspiration would produce collapse. I have used chloroform in double empyema in children, and where struggling and coughing must be reduced to the minimum. In adults, however, with whom one can reason, I believe that ether is safe and preferable in a large proportion of cases. I have used it without evoking any symptoms to cause anxiety in patients with deep cyanosis, delirium, muscular twitching,

intermittent pulse, and excessively rapid respiration; in a woman seven months pregnant, with deep cyanosis and a weak, rapid pulse; in a man with the left chest so full of pus that the spleen was pushed down to the ilium, and so distressed in his breathing that he could not lie down. On the other hand, one case, a man, profoundly poisoned with streptococcic absorption, took ether badly, the stage of excitement being marked, and undoubtedly in this instance the operation should have been done under local anæsthesia, but such a case is rare. The objection to the use of cocaine as a routine means of producing anæsthesia is the mental shock and fright added to the peculiar condition of mental distress that is so characteristic of this disease; the struggling, which is frequently more marked than in cases properly etherized, and the not infrequent imperfect anæsthesia produced. The struggling that attends primary anæsthesia with ether is easily controlled, of short duration, and is not due to suffering.

Expansion of the Lung. To insure rapid expansion of the lung too much cannot be said to induce physicians to submit their patients to early operation. Were this principle followed more strictly than it is, such operations as those devised by Schede and Estlander would rarely be required. One cannot tell beforehand what type of case is going to expand early; and although occasionally a patient is seen in whom, after conservative treatment, rapid expansion follows a late operation, these cases are exceptional and are dangerous precedents. To have the compressed lung follow one's finger to complete expansion as the pus flows from the chest is most satisfactory both to the patient and the surgeon, while to submit a patient, after months or years of treatment for a persistent sinus, to a severe operation, a long convalescence, and a deformed and inadequate chest is disheartening, to say the least. I have doubts of one's ability to foretell what lung will expand rapidly in all cases. Of two adults with streptococcic empyema following pneumonia, operated upon within a few days after the onset of the empyema, where the conditions were as nearly alike as pos-

sible, the lung in the one case followed the evacuation of the pus and expanded to completion at once, while in the other the lung refused to expand to any extent by the end of a week, when death took place from pneumonia of the opposite lung. In another streptococci case, treated expectantly for two months, the lung steadily expanded in a few weeks after resection of a rib; but this is an exceptional case, and an unsafe example to follow. I have had delayed expansion or incomplete permanent expansion in both pneumococcic and streptococcic empyemata, but where the operation has been done soon after the formation of pus, immediate expansion has taken place in both types, though, as a rule, complete expansion is rather apt to be slower in the pneumococcic cases—that is, full resonance is not noted before several weeks. The cases of incomplete expansion where operation has been delayed are usually those of mixed infection, and the severer secondary operations have been resorted to after long and tedious drainage.

It is a well-established rule that pus should be evacuated as soon as it is discovered, and yet I know of no place where this principle is more applicable than in cases with pus in the pleura—a principle that in my experience is constantly violated by practitioners of large experience.

It should be borne in mind, also, that incomplete expansion may come from imperfect drainage, from relapse consequent upon a return to a life of hardship or dissipation too soon after the healing of the wound, and from an accident that occasionally happens, the loss, unknown to the patient and surgeon, of a drainage tube or a sponge in the pleural cavity.

The sinuses that persist in tuberculous patients form a class by themselves, and are not considered here.

Cause of Death. The deaths in my cases have come from dislodgement of an embolus from hearts displaced by a left pleural cavity filled with pus, from a relapse or an extension of a pneumonia, and in the streptococcic cases from a rapid general infection. In the first class of cases, especially

if of long standing, one or more preliminary aspirations, to reduce the distention gradually, are safer; but in recent cases it seems sufficient to evacuate slowly at the operation and then to enforce absolute rest for a few days. The streptococcic cases occasionally are so rapidly fatal that, no matter how early the cavity is emptied, nothing is of avail; but in those not so acutely poisoned, where there is a steady and persistent absorption, the chances of recovery are far less good after a late than after an early operation; and this possibility is very apt to be overlooked by those who have had but little experience in pyæmia and septicæmia.

Technique. The operation for empyema is well described in all text-books, but I wish to suggest some modifications that have proved useful to myself, and which may be of some help to the practitioner who is called upon but rarely to undertake the operation.

I make it a rule to explore with a trocar, for diagnosis, as soon as the patient is anæsthetized.

In old, left-sided cases with marked displacement of the heart the pus may be withdrawn by preliminary aspiration, to avoid the risk of embolism.

I rarely open between the ribs. Drainage is not so satisfactory, and, unless there are urgent indications to the contrary, resection of a rib is practically as safe, and in other ways is better surgically in the largest majority of cases.

The incision, especially in cases requiring rapid operation under primary anæsthesia, is best made at a right angle to the rib. There is no danger of missing the rib selected; it can be reached at one stroke, and plenty of room is allowed for resection. Practically, I now use this incision in all cases.

It is not necessary to resect the rib at the lowest level. Personally I prefer the seventh or eighth; the cavity usually found is just as well drained at this level, and there is no danger of wounding the diaphragm.

In cleaning the periosteum from the rib I prefer a periosteum elevator without a cutting edge—the handle of a knife

or a pair of scissors curved on the flat is equally good—because the danger and annoyance of wounding the artery is reduced to a minimum.

The cavity drains equally as well if made in the anterior or midaxillary line as when made far back where the rib is more difficult of access.

The drainage-tube should be short; it should not project beyond the inner surface of the chest. The cavity drains better, and the irritation of the lung pressing on the end of a long tube is obviated.

Where a suitable operating-table is at hand the operation may be done with the patient half sitting up, the arm of the affected side being held over the head. In this position free respiration is allowed to the healthy side, and free access is given to all but the furthermost portion of the chest posteriorly. Moreover, most patients breathe more easily in this position.

After the operation the patients improve more rapidly if they are allowed to sit up as soon as they can do so safely. Systematic lung and chest exercises, climbing hills, running, etc., are all valuable aids in expanding the lungs. The pneumatic cabinet, and in obstinate cases removal to a high altitude, should be of value also, though I have had no experience to verify the latter suggestions.

DISCUSSION.

Dr. Harold Williams: I should like to ask Dr. Munro in regard to the use of anæsthetics in children. Dr. Holt, in his work on *Pediatrics*, says the employment of general anæsthetics is exceedingly hazardous in empyema of children, and mentions the fact that he has seen four deaths resulting from this employment. If Dr. Munro is able to administer anæsthetics in such cases it seems to me a matter of the utmost importance and one which should be widely known.

I would suggest that perhaps the unfavorable results which have been reported were due to the manner in which the anæsthetic was given rather than to the anæsthetic itself.

Dr. V. Y. Bowditch: I agree with the reader of the paper so thoroughly that I have little or nothing to add. It has always been my custom to refer the case to a surgeon for operation at once if pus is found in the pleural cavity, unless it be a case of pyo-pneumothorax in conjunction with advanced phthisis, in which case I question whether a permanent opening is advisable, from the fact that it is merely an additional source of discomfort to a hopelessly ill patient, and occasional aspiration may be sufficient to relieve urgent symptoms.

With regard to the use of the pneumatic cabinet, I have had no experience with it in empyema, but it suggests itself as a valuable adjunct in the expansion of the lung after evacuation of the pus. I should like to hear from those who differ from Dr. Munro about the advisability of immediately operating when pus is found.

Dr. Curtin: Speaking of expansion of contracted lungs and stretching of old adhesions after an attack of pleurisy, there is, in my opinion, nothing better than horseback riding, with, if possible, a sojourn in some high altitude, where the rarefied atmosphere will cause the air vesicles to become distended.

Dr. Elsner: I recently saw a child with an empyema of some months' standing which had been unrecognized, and finally broke into the lung. The right side, on which the empyema existed, was flooded with pus, particularly when the child turned over on the left side; then it rapidly became cyanotic. The operation was done from below, the child on its back. This obviated the necessity of turning the child.

Dr. Jacobi: Children should never be given ether. They should always be given chloroform.

Dr. Munro: I am willing to admit that chloroform at times may be preferable to ether for children, and I suppose we employ the latter chiefly because we have become accustomed to it.

In reply to Dr. Williams, I would say that I firmly believe that if ether is given by a skilled anæsthetizer it not only in a good share of cases does no harm, but acts as a stimulant, is not disagreeable, and has practically no after-effects. If given carelessly or ignorantly it is dangerous, and possibly more dangerous than chloroform. When given properly for operation in the primary stage of anæsthesia it is superior, in my experience, to any other form of anæsthetic.

I wish I could unburden my soul regarding those of my professional brethren who have left empyemata go for weeks and weeks, knowing that there was pus there and waiting for something to turn up.

TRAUMATIC RUPTURES OF THE HEART, WITH A CASE.

Synopsis:

1. History of a Case of Rupture of the Wall of the Right Ventricle in a Young and Vigorous Man by a Bicycle Accident, without Penetration of the Chest Wall or of the Pericardium.
2. A collection of sixteen similar cases from literature since Gamgee's and Fischer's lists were published, about 1870.
3. A consideration of the mechanical causes of the accident and of its extreme rarity.
4. The rôle of operative surgery in traumatism of the myocardium.

By RICHARD COLE NEWTON, M.D.,
MONTCLAIR, N. J.

F. M., aged twenty-eight years, American, born of German parents, carpenter.

This young man was a wheelman of some experience, and was reported to have ridden in some races. On September 19, 1898, at about 5.10 P. M., he was riding his bicycle rapidly, when the front wheel came in collision with a stout rubber and canvas hose, four inches in diameter, which was lying on the ground across the road, and was distended with water, which it was conveying under pressure from a hydrant to a water car.

There were only two or three eye-witnesses of the occurrence, and their accounts of it differ somewhat. There seems to be no doubt, however, that the so-called head of the wheel was broken near its junction to the fork by the force of the collision, and the rider was thrown up a foot or two in the air and fell heavily near his broken wheel. It is probable that he kept hold of the handle-bar and took it with him

when he left the saddle. As he struck the hard, macadamized road the handle-bar was turned over, and its post, a straight steel rod, six or eight inches long and an inch in diameter, was interposed between his body and the ground, and consequently struck him with great force in the chest. He got up, holding his hands to his left side, staggered a few feet, and then fell down in the road, where he remained groaning and writhing with pain.

Dr. Wilson, of Bloomfield, saw the man a few minutes after the injury, and found him collapsed, cold, and sweating. He was nearly pulseless, and was lying curled up and in great pain. He was partly conscious, but gave no clear account of himself. A hypodermic of brandy was given him, which seemed to revive him somewhat. No marks or bruises were detected on the body or the head. In the meantime, a wagon having been brought, the man was removed as carefully as possible to the Mountainside Hospital. Before he arrived there he revived enough to tell his name and residence.

He was still in great pain, and had a tendency to throw himself over onto his left side. He did not vomit nor raise any blood, nor did the bowels or kidneys act. After he had been carried into the hospital the writer, who was at the time the surgeon on duty, was summoned. The man's pulse was then 78, and moderately strong. He called for water, but was able to swallow only a very little. There were no convulsions. Before the writer arrived at the hospital the man had quietly died, at 6.45 P.M.—a little more than an hour and a half after the fall.

Dr. Washington, the county physician, was notified, and about 11 o'clock the next morning he viewed the body and directed a partial autopsy.

Autopsy Seventeen Hours After Death. Body that of a well-developed muscular young man, about five feet six inches in height, weighing about one hundred and forty-five pounds. Rigor mortis marked. Two or three unimportant bruises were noted on the shins and about the knees. A small, semi-

circular, freshly made, depressed mark was seen on the skin over the sixth rib, about half-way between the nipple line and the sternum on the left side. This had apparently been inflicted by a piece of tubing or other hollow cylindrical body, about an inch in diameter. A small depression, about an inch internal to this mark, was noted, as though one of the costal cartilages had been fractured and depressed, and pressure at this spot showed that this lesion had occurred. When the chest wall was opened nothing abnormal was detected except the fracture of the sixth costal cartilage, near its junction to the sternum, and some laceration of the intercostal muscles. The pericardium was intact, but was somewhat distended. When it was opened it was found to contain from eight to ten ounces of dark, clotted blood. When the heart was lifted up its cavities were partly distended with blood. Its weight was eleven and one-quarter ounces after all the blood had been washed out of it. A transverse rent was discovered at the apex of the right ventricle, extending through its wall. The tear had partly separated a triangular flap of the heart substance. The measurements of the flap were as follows: From the apex of the heart to the upper extremity of the posterior tear, one and one-fourth inches; from the apex to the upper extremity of the anterior tear, one and five-eighths inches; and at each of these extremities the epicardium was torn several lines further than the muscular tissue.

This tear of the epicardium was more extensive on the anterior aspect. On turning up the flap the rent measured from side to side externally one and one-half inches, and internally through the endocardium three-eighths of an inch. The internal rent was immediately contiguous to the intraventricular septum. In other respects the heart walls and valves were normal and competent.

A photograph of the heart was kindly taken for me by Dr. Henry Power, and is reproduced herewith.

The cause of death was evidently the cardiac rupture, apparently produced by the same force that had fractured the sixth costal cartilage.

Showing triangular flap of the tear at apex of right ventricle pinned back.

Rupture of the heart from contusion without penetration of the chest wall is a rare accident.

Gamgee had collected twenty-eight cases when Ashhurst's *Surgery* was printed, in 1871, and this number is quoted in Dennis's *System of Surgery*, printed in 1895, and in Gould and Pyle's *Anomalies and Curiosities of Medicine*, printed in 1896. Gamgee's list was also referred to by Mr. Cecil Robertson in 1897,[1] who, however, placed the total at twenty-two.

So it seems certain that this accident must be very rare if there have been no additions to the published list in twenty-five years. My impression, however, is, after a partial review of the medical literature for that period, that the next compiler can considerably augment if not double Gamgee's list. This, however, only confirms the previous observations upon the rarity of the accident.

Rupture of the heart from any cause must be a very infrequent mode of death.

As, for example, Kouskoff[2] reports that he has only observed three ruptures of the heart in 8000 autopsies. In the West Riding Asylum, Wakefield,[3] only one rupture of the heart was noted in 4516 deaths. This last was a spontaneous rupture due to disease of the heart muscle. And of the three cases observed by Kouskoff, one at least, and probably all, were due to similar causes, and were in elderly people. I have, however, found a few recent cases of death from rupture of the heart from a bruise or kick, some with and some without fracture of the ribs or sternum, without penetration of the chest walls, which are of so much interest that they will bear recapitulation.

1. A lad of sixteen[4] years was caught between some wooden railings and the shaft of a trap drawn by a runaway pony. He lived a month, and an autopsy revealed no injury to the superficial tissues or the ribs. A rupture one-third of an inch long was found in the posterior aspect of the left ventricle. The myocardium was perfectly healthy except at the point of rupture. The accident had caused a partial rupture of the inner portion of the left ventricular wall opposite the spot where

the wagon-shaft had pressed against the breast. This had developed into a cardiac aneurism which had subsequently burst.

2 and 3. Vestberg[5] gives two cases of aneurism of the heart due to traumatism in a collection of sixty cases of cardiac aneurism, published in the *Nordiskt Med. Ark.*, January 10, 1898.

4. J. B. Gibbons[6] gives the following case in the *Indian Med. Gazette:* The patient, a cooly, thirty years old, was struck across the chest with a bamboo walking-stick. He fell to the ground and vomited. He was taken to a hospital, where he died three hours afterward. It was thought that he had sustained a fracture of the spleen. This viscus was found upon autopsy to be intact. The pericardium, however, was distended with a quantity of blood, estimated at about fourteen ounces. The heart was contracted, and there was a small, irregular-shaped rupture in the apex communicating with the right ventricle. The muscle fibres appeared healthy, and the wall of the ventricle was of the usual thickness except at its apex, the point of rupture, where the wall was unusually thin. There were no signs of myocarditis, either recent or of old standing. The arteries and valves of the heart were quite healthy.

The stomach contained forty-eight ounces of rice and water. The organs generally were healthy. The reporter observes that ruptures of the healthy heart are comparatively rare injuries, even in cases where the chest has been submitted to great violence, and, when present, are commonly associated with fractures of the ribs or sternum, and often with ruptures of other organs, such as the lungs, liver, or spleen.

The chief peculiarity of this case is that a blow with an ordinary walking-stick, hurriedly struck by a man of poor physique, should have ruptured the heart—an organ which generally escapes injury even when the thorax has been subjected to great violence.

5. Hutchinson[7] reports that a farm laborer, aged fifty-nine years, was brought to a hospital at mid-day, having sustained

a kiek from a horse on the forehead and the chest two hours before. A hæmatoma oeeupied almost precisely the præeordial area. The skin was unbroken, and there was no evidence of fracture of the ribs. The extremities were very cold. Pulse 60 and regular, but of small volume and low tension. At 12.20 the patient was suddenly attacked with severe pain in the cardiac region and between the scapulæ. He complained much of want of air. The pain was relieved somewhat by hot fomentations. In his struggles for breath the man nearly jumped out of bed, and on being restrained seemed to derive some comfort by lying on his left side. He died at 2 P.M. On autopsy no fraeture of the ribs was found and no ossification of the eartilages. A rent of the pericardium was noted anteriorly and a second one eommunieating with the pleural eavity. The heart was lying in its normal position and in diastole. A rupture was discovered at the extreme apex of the right ventricle. This was direct and not valvular in character. The eavities of the heart contained no blood. The valves appeared quite healthy. The left pleural cavity, however, contained eonsiderable blood, causing partial eollapse of the left lung. There was slight atheroma of the aorta, and the heart muscle looked pale. There was no evidenee of disease or injury elsewhere. The reporter says that this man had probably lived four hours, because he had a rupture of the pericardium and adjacent pleura whieh permitted the escape of the effused blood from the pericardial sac, thus preventing its overdistention and the eonsequent eompression and rapid stopping of the heart.

6. Dr. Bennett[8] reports a case of rupture of the heart from explosion of a bomb. There was no rupture of the perieardium. The man lived three hours.

7. M. Terrillon[9] reports that a lad, aged twelve years, died four hours after a fall with a tear of the cardiac wall.

8. He also reports that a man, aged twenty-one years, was kicked by a horse in the ehest. He got up and walked toward the stable, but fell dead after taking a few steps. There was fraeture of the sternum, although there was no

outward appearance of the blow. The pericardium was intact, and was filled with yellowish serum and coagulated blood. There was a rupture one-half inch long in the right ventricle, also a fissure in the intraventricular septum and an incomplete tear at the circumference of the auriculo-ventricular orifice.

9. A case of traumatic rupture of the pericardium and both ventricles from fracture of the sternum, no ribs or costal cartilages having been broken, is reported[12] in the *British Medical Journal*, October 14, 1893.

10. Dr. Anskoff[13] reports a case of a mechanic killed by an explosion. Autopsy showed fracture of the third rib and of the fifth costal cartilage. The left wall of the pericardium was torn, but the auricles were intact, whereas both ventricles were torn, also the intraventricular septum. There were no ecchymoses on the cardiac walls. The endocardium was almost intact, but it was transparent at the apex of the left ventricle. The papillary muscles and the tendinous cords were pulled from their attachments in the left ventricle. The valves were intact and presented no appreciable change to the naked eye.

According to Dr. Anskoff, rupture of the heart in this case may be compared to that of a balloon filled with water to which a violent blow has been given.

11. Mr. Cecil Robertson[1] reports in the *Lancet* a case of a man, aged forty-nine years, apparently healthy, who opened the door of a railway carriage just as the train began to move and fell somewhat heavily, striking his left shoulder on the ground. When asked if he was hurt he replied "No," and proceeded to walk some two or three hundred yards to his work, where he died three-quarters of an hour after his fall from the train. The pericardium was intact, but distended with a mass of blood-clot and fluid blood. On the front of the left auricle was a rent one and one-quarter inches long. The wall of the auricle was very much thinned at the point of rupture. The ventricles were hypertrophied. The heart weighed twenty-two ounces. Beyond a slight abrasion over

the right knee there were no external marks of violence, and there is little doubt that the cause of the heart rupture was the violent impact of the left shoulder upon the platform.

12. Whitaker[14] says that Ward reports a rupture of the heart from external violence without breach of the skin.

13. And Fischer the rupture of an apparently sound heart.

14. And Dransart gives a rupture of the heart without involving the endocardium.

15. I clipped the following from the *New York Sun* of April 2, 1899: C. McC., aged seventeen years, received a blow over the heart while boxing. He sank to the ground and died in ten minutes. On autopsy he was found to have an enlarged heart, which had burst.

16. Dr. John H. Larkin reported to me verbally the case of an Italian laborer who was struck in the breast by a flying missile and died in a few hours. The autopsy showed a rupture of the heart without penetration of the chest wall.

Of Gamgee's[23] twenty-eight cases of traumatic heart rupture, in nine there was no fracture, and "either no bruise of the parietes or a very slight one." The pericardium was intact in at least one-half of the cases, and in twenty-two in which the precise seat of the lesion was noticed the right ventricle was injured in eight, the left in three, the left auricle in seven, the right in four.

The longest period which any patient survived the injury was fourteen hours.[10]

The peculiarities of my case, which render it nearly or quite unique in medical literature, are: 1. That it is, so far as can be ascertained, the first rupture of the heart due to a bicycle accident. 2. That I can find no record of a case in which a blunt instrument fractured one costal cartilage and drove the fractured extremity of that cartilage through the apex of the heart without tearing the pericardium. 3. Had the site of the blow or its direction been slightly altered the heart rupture would not have occurred, and therefore the chances of the same accident occurring again seem infinitely remote.

A consideration of the mechanical aspects of this accident

shows that the heart was probably in systole when struck, since, as M. Terrillon has pointed out, if the blow be received in systole the contracted state of the cardiac muscle fibres predisposes the rupture to take place at the point struck, probably the right ventricular wall, and the shock, if of sufficient force, may affect the pillars and septa and cause there also a solution of continuity; whereas if the blow be received while the heart is in diastole the heart cavities are filled with blood and are communicating, so that eccentric compression may produce a tear in the resisting valves or the septum.

In speaking of this aspect of cardiac rupture, Prof. A. F. Holmes,[11] after reporting an instance of rupture of the wall of the right ventricle without a tear of the pericardium, in a case of gunshot-wound of the chest, says: "Entertaining, therefore, no doubt that the wound was caused by the direct contact of the ball driving the pericardium before it, I think that the manner of its formation may be more readily understood by supposing that at the instant of being struck the heart was in the act of contraction, its fibres hard and rigid from their muscular action. In this state the ball suddenly impinging produced an effect similar to what happens to an overbraced harpstring when struck—the fibres snapped across."

In Prof. Holmes' case the man when shot was making strenuous exertion, endeavoring to force his way against armed opposition up some steps and into a house. He was a young man, aged eighteen years, and, so far as known, in good health. His heart was also healthy. In all these respects the case resembled mine. My patient was young, strong, and vigorous, and at the time of the injury was making powerful exertion. Whether he tried to "jump" his bicycle over the extended hose, as is sometimes done, or not, cannot be determined; but his heart surely was pulsating with great vigor, and as the apex was pushed upward and toward the chest wall in systole it met the fractured end of the broken costal cartilage, which was driven against it with great force, and the rupture resulted. As the unfortunate

man was only impaled upon his detached handle-bar for an instant, this force was only momentarily exerted, which, in additon to the limited power of movement of the cartilage (restrained by the muscular and tendinous structures of the chest wall), and to the fact that its extremity was smoother and softer than the end of a broken bone would have been, may account for the limited extent of the heart rupture and the non-penetration of the pericardial sac.

If any one believes that this rupture could have occurred from within outward, a glance at the heart itself will show that the anterior or external leg of the triangular tear is longer by three-eighths of an inch than the posterior or internal leg, which is strongly confirmatory of the assumption that the wound was caused as described by the exterior fragment of the broken costal cartilage of the sixth rib, pushed inward and upward by the external force. I make the above statement, although I am aware that Fisher[24] remarks in his celebrated essay, "That ruptures of the heart caused by either external or internal violence have a similar appearance, so that from the anatomical changes no conclusions can be drawn as to the mode of injury. When the thorax has been subjected to severe concussion or violence the appearances are the same as where fracture of the rib or ribs has given rise to the rupture, or almost so."

And, lastly, my case is peculiar in this respect, that the heart seems to be quite healthy and normal, whereas in nearly all the reported cases of ruptured heart from contusion of the chest wall there was some atheroma, hypertrophy, or other abnormality or disease present. Even the cooly (Case 4), whose heart was healthy in other respects, had a congenital abnormal thinness of the apex of the right ventricle. Nevertheless, had he not eaten so freely of rice that his heart was forced against the chest wall by distention of his stomach, the stroke of a bamboo cane would not have ruptured it.

Cunningly as nature has done her work in surrounding the "citadel of life" with many safeguards, and affording it also a marked degree of mobility, so that it may escape from very

severe, crushing wounds of the chest, occasionally, as it were, nature is caught off her guard by some peculiar and unforeseen combination of circumstances, in which a usually harmless amount of force does an entirely unexpected amount of damage. Firm and powerful as the healthy heart muscle is, it will sometimes be subjected to a pressure which will rupture it if exerted while the fibres are tense and rigid, as in systole. Of the cases I have quoted I think that the entire sixteen may be added to Gamgee's list, which, with my own case, make forty-five in which heart rupture has occurred from traumatism or violence without penetration of the chest wall, and with generally little injury to the overlying parts.

All of the cases seem to have been fatal, whereas a large number of stab-wounds and bullet-wounds of the heart have recovered. It surely seems scarcely credible that so severe an injury as rupture of the heart wall from contusion can be recovered from. Fothergill[15] says: "There is no recorded case of the healing of a rupture, but, on the other hand, according to Velpeau, even a wound which penetrates the entire thickness of the ventricular wall may heal by cicatrization."

Still it is conceivable that my case could have been successfully treated surgically had a positive diagnosis been possible immediately after the injury. At all events, Rehn[16] reports a successful case of suture of a wound of the right ventricle 1.5 cm. long.

And Williams[17] reports a case in which the pericardium and heart wall were both wounded. He successfully stitched up the former.

Capellen's[18] case of suture of the heart wall and pericardium lived two and one-half days. It has been found that the heart will bear sewing of its walls very well;[19] and also that aspiration of the right ventricle may not only prove to be harmless, but may be of positive benefit. As, for example, a case reported by Sloan[22] and another by Roger. Other cases in which heart puncture and aspiration were resorted to are reported by Westbrook, Dana, Colwin, Evans, Bouchut, and

Fischer. "In some instances temporary relief was afforded; in all no harm was done" (I quote an editorial in the *Medical Record*).

A number of experiments have been performed to ascertain, if possible, the exact amount of injury which can be inflicted upon the heart and repaired surgically without killing the animal. Some of these have recently been reported from Breslau,[20] in which sutures were passed even nearly around the heart, without doing serious damage, if secondary injuries were avoided. One-third or even one-half of the ventricles could be excised after applying a proper suture above, and the ventricles and even the intraventricular septum might be freely incised and sutured. All these assaults upon the hearts of small animals were survived in four cases out of six when severe hemorrhage could be avoided.

Verily, as the wise Otis[21] said years ago: "We are still ignorant of the degree of injury the organ may sustain without destruction of life, and can only conjecture the causes of delay in the termination of some rare cases presenting lesions that are generally instantly fatal."

The last point which seems worthy of note in my case is that the man revived perceptibly from the shock of the injury, When seen by Dr. Wilson his pulse was very bad. It improved somewhat after a hypodermatic injection of brandy, and after the hospital was reached it was reported to be quite strong. The man's mental condition also cleared up so that he gave his name and address correctly. This shows, I think, that he was coming out of shock, and that his death was due to the gradually increasing effusion of blood in the pericardium. The causes of immediate death, we are told,[18] after heart injuries are two in number—nervous and mechanical. The first acts by interference with the ganglia presiding over the movements of the organ, and the second by the effusion of blood in the pericardial sac. For the former lesion surgery is of no avail. For the latter, however, it might be of obvious advantage. Inasmuch, however, as in ruptures of the heart from traumatism both of the morbific causes mentioned are

apt to be effective, the part that operative surgery can play in this class of cases must always be an uncertain factor. Another obstacle to successful interference in such a case as mine would be, of course, the difficulty of making a positive diagnosis. In cases where there is laceration of the chest wall a portion of the pericardium may be laid bare, so that it can be inspected. In such a case it might safely be incised and the heart examined, and, as has been occasionally done, wounds of its substance might be repaired by sutures.

Elaborate directions for operating on the heart have been given; but, from the difficulties and uncertainties surrounding the diagnosis of heart traumatisms, not to speak of their extreme rarity, the benefit to be expected from operative interference must be, generally speaking, quite problematical.

Bibliography.

1. Journal of the American Medical Association, March 13, 1897, p. 508.
2. American Journal of the Medical Sciences, vol. iii. p. 727.
3. British Medical Journal, November 16, 1895.
4. Journal of the American Medical Association, August 28, 1897, p. 449.
5. Ibid.. March 12, 1898, p. 626.
6. New York Medical Journal, January 15, 1898, pp. 103 and 104.
7. British Medical Jonrnal. December 22, 1894, p. 1427.
8. Ibid., November 16, 1895.
9. American Journal of the Medical Sciences, October, 1879, pp. 566 and 567.
10. Anomalies and Curiosities of Medicine, p. 625.
11. The British-American Journal of Medical and Physical Science, December, 1845.
12. British Medical Journal, October 14, 1893.
13. New York Medical Journal, May 23, 1896, p. 961.
14. Twentieth Century Practice, vol. iv. p. 353.
15. The Heart and Diseases, p. 244.
16 Medical Record, May 29, 1897, p. 790.
17. Ibid., March 27, 1897.
18. Ibid., February 27, 1897.
19. Year Book of Treatment, 1898, p. 185.
20. Journal of the American Medical Association, December 10, 1898, p. 1436.
21. Medical and Surgical History of the War of the Rebellion, vol. i. p. 530.
22. Medical Record, March 30, 1895, editorial.
23. Principles and Practice of Surgery, Ashhurst, edition 1871, p. 356.
24. Die Wünden des Herzens und des Herzbeutels, Berlin, 1868.

DISCUSSION.

Dr. Roland G. Curtin: I recall a case, which I wish to put on record, which is in some respects dissimilar to the one reported by Dr. Newton.

A man had been for years an inmate of the surgical ward of the Philadelphia Hospital. He was nearly sixty years old, with a tendency to melancholy. One day he threw himself down between the stairs from the fourth to the first floor, and was picked up dead. On autopsy it was found that he had enough injuries to kill half a dozen men. No external rupture of the heart was found on autopsy, but the intraventricular septum was split for nearly two inches. Microscopical examination of the heart showed marked fatty degeneration of the muscular fibre.

I believe that in these cases of rupture the muscular fibres will generally be found fattily degenerated; but some rare instances may occur where there has been a rupture from slight injury, and the only cause to be found is an abnormal thinning of the heart muscle.

FUNCTIONAL CARDIAC MURMURS.

By A. JACOBI, M.D., LL.D.,
NEW YORK.

Organic cardiac murmurs have always been distinguished from the accidental and the functional. It is self-understood that the first named are caused by actual, mainly valvular, diseases, and will remain as permanent as the anatomical changes which produce them. Accidental[1] murmurs should be called those which, though they appear to be cardiac, do not result from actual cardiac disorders. They may even be extracardial, and sometimes require great attention and repeated examination before their true character can be ascertained. They depend on adhesions between the pleura and pericardium, with or without those between the two pericardial layers; are not transmitted through the blood-current, not always rhythmical, not quite synchronous with the contractions of the heart, and not of equal strength and audibility. Some, according to their origin, are superficial, some distant, grating (pericardial), or soft (pulmonary), increase during expiration, and may stop altogether when breathing is intermitted. They are seldom heard posteriorly.

Changes in the shape of the chest-wall, such as annoy the heart, alter the character of the cardiac sounds. In three cases of rhachitical infraction of ribs Hochsinger observed distinct

[1] Dr. George W. Webster, of Chicago, in a paper read before the American Medical Association of 1899, proposes to do away with the discrimination of "accidental" and "functional" murmurs and to employ the former adjective exclusively. He claims that "it commits us to no theory of causation, indicates no pathology, and avoids a discussion of the question whether functional disturbances occur with pathological changes." But it is exactly this discussion which is urgently required. Without it the difficulties surrounding the etiology and nature of inorganic murmurs will not be overcome.

cardiac murmurs; Steffen the same in the common forms of rhachitical deformities of the chest. These forty years I had many opportunities of publicly demonstrating, with or without apparent hypertrophies of the heart, more or less marked cardiac murmurs attributable to the triangular or quadrangular shape of the rhachitical thorax, the walls of which, being no longer elliptical, touch a large surface of the heart. These murmurs are not always the same. Within a few minutes the well-marked cardiac murmur of a baby sitting erect or bent forward may change into a muffled sound when the patient lies down—the best proof of its resulting from the mechanical annoyance on the part of the chest-wall. Indeed, this muffled sound and the murmur differ only in degree. The former may often be produced by the pressure of the stethoscope on the flexible ribs of the young. Such observations, as noticed by Henoch, and also by Hochsinger, who, however, speaks of a coarse heart-sound only, may readily be verified, provided the age of the patient is taken into account. They will be the more positive the younger the baby and the more flexible the ribs. Now and then the cardiac sound may be changed by pressure over the pulmonary artery.

"Functional" should be called all those murmurs which cannot be explained by some anatomical alteration of a valve or of the myocardium. The causes, however, which are responsible for the exhibition of functional murmurs are altogether too numerous. To say that no single theory covers their etiology is not doing justice to the case. Indeed, there are but few conditions of the heart and bloodvessels to which "functional murmurs" are not traced back. Thus the imagination, or the diagnostic skill—or its absence—of the writer had always a great deal to do with their alleged nosogeny. Protracted diseases and convalescences, losses and abnormal condition of the blood, all forms of anæmia, chorea, poisoning by alkalies or by acids, acute intoxications and long-continued eruptive fevers, septic processes, irregular contractions of the myocardium, degeneration of papillary muscles, minute disturbances of valves or of bloodvessels, chronic

myocarditis, fatty degeneration, are all charged with causing "functional" murmurs. Nervous influences also come in for their share. Undoubtedly strong emotions, excitement, mainly in the young and those very impressible, influence the heart in the most various ways, from temporary palpitation to change of structure. Prolonged emotional strain certainly has that effect, even to the extent as to cause distention, dilatation, and hypertrophy, through prevention of complete systolic discharge equally with physical overexertion. Before and after violent exercises of athletes Schott could discover under the Röntgen rays the different degrees of distention. This momentary distention, when exertion is demanded of an enfeebled heart, though otherwise healthy (for instance, in chlorosis), may lead to persistent dilatation. When the heart is no longer healthy, however (for instance, after infectious diseases, or in fatty degeneration, or in the various degrees of other myocardial changes), both distention and dilatation are more readily established. Nor are pathological alterations required to facilitate their development; for here fatigue, physical, emotional, or mental, renders muscles more flaccid and favors distention. As far as the heart is concerned, its muscular labor depends, moreover, on the amount of support it finds in neighboring organs. The inability of lying on the left side, which is experienced by most healthy people, is caused in this manner. In that position the heart is more flaccid and requires more exertion to overcome resistance, a fact which is best shown by the increase in the number of respirations of from 50 to 80 per cent.

Functional murmurs are described as soft and low, short or long, not always blowing, and are frequently combined with, or are the terminations of, a more or less normal heart-sound. In almost every instance they are systolic; in the adult they are mostly aortic, and then audible in the carotid; in the child they are more frequently found over the pulmonary or over the pulmonary and mitral regions. In regard to the locality and extent of their audibility there have been many differences of opinion; still, there

appears to be unanimity in regard to their inaudibility posteriorly in almost every case. A functional murmur may persist for weeks and even months, but it has not the uniform quality of an organic murmur. It is more or less soft, or loud, or prolonged. Many disappear quite rapidly, or, after having vanished, return. In this respect they differ widely from organic murmurs, which are more persistent as far as time and character are concerned. It should, however, not be forgotten that organic valvular murmurs may disappear either through recovery from endocarditis or through the establishment of compensation. But in these cases they diminish in loudness and duration very gradually only. Nor should it be overlooked that an increased frequency of the pulse, with its frequent and insufficient contraction of the heart muscle, and thin bloodvessels, and shortened valve excursion, conceals a murmur which was present when the pulse was slow, or which returns when a cardiac stimulant reduces the number of heart-beats.

Duplicated sounds should almost never be taken to be either accidental or functional. They are nearly always organic and of more value than Leube appears willing to assume, both the splitting of the second sound (gallop rhythm—**V V**) and that of the first (**V V**—"rappel" of the French). The former is often observed in aortic stenosis, in chronic nephritis, sometimes also in conditions of utter exhaustion, and in bad cases of chlorosis; the latter in mitral stenosis, sometimes with oliguria followed by polyuria. Both of them are rare in infants and in the very aged. A fine specimen of the last anomaly has been under my observation (Charles G., ten years old, with the diagnosis of mitral stenosis and chronic myocarditis) in my division of Roosevelt Hospital.

Vascular murmurs should not easily be mistaken for cardiac; as a rule, they are transmitted. They are very rarely confined to the arteries of the neck, either in the adult or in the young. The relatively large size of the carotid in the young, mainly in the rhachitical young, with its lowered blood-pressure, may give rise to an occasional soft murmur.

This infantile condition of the carotid (and basilar) artery accounts for the murmur which is often audible over the open fontanelle, and was (rather erroneously) attributed by Fisher (Boston, 1835) to rhachitis only; it is quite possible that the irregular shape of the rhachitical carotid canal contributes to the murmur, which has always been mentioned among the "functional." If, however, the large size of the artery, with its consecutive diminution of blood-pressure, or an anatomical change in the carotid canal, or both, cause the murmur, to what extent should we be justified in calling functional the murmur which is due to such tangible anatomical causes?

Venous murmurs should never be mistaken for those originating in the heart. They are frequent, mostly about the chest and neck, and generally found in anæmic adults, less so in anæmic children, still less in infants, and never, it appears, in babies suffering from atrophy. The jugular vein is a frequent seat of murmur, particularly when the bulbus v. jugularis is large compared with the size of the vein; in these cases the murmur is explained by the formation of a vortex. The v. anonymæ also exhibit murmurs, which are combined with those of the jugular, are heard on both sides of the sternum, and are not isochronous with the sounds of the heart. When such murmurs are complicated with those of the apparently normal heart in adults, it is mostly safe to claim the latter as functional; when in children, and particularly in small children, as organic, for the number of very young children that develop other than organic murmurs is small. That is why while venous murmurs are frequent in pernicious anæmia, leucocythæmia, scurvy, and hæmophilia of the adult, even when cardiac murmurs are still absent or not marked, they are often missed in those of the young child. In fifty cases of infantile scurvy I do not remember to have ever met them. Why should this be so?

The heart of the young is comparatively large, heavy, and healthy. Its weight in the newly born is 0.89 per cent. of the body-weight; in the adult, 0.52 per cent. In the newly born the cavity, however, is small, 23 c.cm., compared with

100 c.em. at the seventh and 140 c.em. at the fifteenth year. Its muscle is massive, equally thick on the right and left sides, the contractions rhythmical and energetic and quite frequent. That is why the valves, which are small and elastic, vibrate easily and quickly. During the first five years there is an increase of the heart in bulk and weight, but none in circumference. That is why, while the area of dulness in early age is extensive, the impetus is quite marked. The cavities dilate rapidly only after the fifth year, and the large arteries, mainly the carotid and subclavian, lose their disproportionately large size only after the seventh year.

This condition of things prevents a predisposition on the part of the infant heart to murmurs of any kind. Indeed, they are very rare in the first four years. In regard to this fact, which was clearly stated by me in 1888,[1] the authorities do not always agree. Fifty years ago Charles West expressed the opinion that they were frequent, but it is very probable he mistook or meant vascular murmurs. Gerhardt thinks they are rare; Biedert and Steffen, however, frequent. Bouchut believed them to be very frequent, under the impression that what he described as a proliferating endocarditis ("endocardite végétante") in the newly born must neeessarily cause murmurs. What he so denominated was, however, nothing but Albini's valvular nodes, or the "blood-cysts" of Luschka and of Parrot, recently again described by Giovanni Berti ("noduli ematici delle valvule cardiache," 1898)—that is, small elevations on the lower side of the valves containing or depending on minute hemorrhages. They are very frequent and liable to disappear, but do not always do so, for I have seen many a case, and followed it up to advanced age, in which those nodules must have been large enough to result in the systolic murmur observed by me which proved persistent. They are apt to be on the mitral valve, are found in the newly born, and persist, and suggest the diagnosis of intra-

[1] Brooklyn Medical Journal, March, 1888: "The heart exhibits functional murmurs but seldom. Whenever there are murmurs present in the infant, it is safe to attribute them to organic disease rather than to mere functional disorder."

uterine heart disease; but are found in the left cardiac cavity, contrary to the rule according to which fœtal inflammation or arrest of development occurs in the right side, and do not result in either dilatation or hypertrophy. Such cases, the like of which I have an opportunity to demonstrate in my clinic perhaps once a year, do not seem to have been observed by Hochsinger when he absolutely and positively denied the occurrence of cardiac murmurs within the first few years of life. Surely these murmurs when found cannot be called functional; they are organic.

Still, these cases are exceptional, and do not controvert the fact that the normal anatomical condition of the infant heart is too powerful to admit the presence of merely functional murmurs. There are but few cases of undoubted functional murmurs in the infant on record. Thiemisch[1] claims one. He tells of an anæmic rhachitic baby of six months that died of pneumonia. A distinct systolic murmur was heard at the apex for a week before death; it was surely not extracardial, for it was distinctly heard in the intervals of respiration, and at the autopsy no valvular lesion was discovered. That is why the murmur is called functional. Still, we are told that the heart was slightly large (may be, within normal limits), and that the muscle of the right ventricle was very pale and flabby. This latter condition means a myocardial anomaly, which is quite capable of rendering cardiac contraction incompetent and irregular when it is localized on one side only. It is more probable that such a limited localization has that effect, while we may imagine that if the myocardial change were universal and equable, the contraction, though feeble, would also be equable.

In more respects than for merely anatomical reasons, the first years of life are peculiarly immune in regard to some of the changes which in advanced life give rise to murmurs. Tobacco, alcohol, tea, coffee, gout, and uric acid have not had time to work; hereditary syphilis does not attack the heart so

[1] Jahrb. f. Kinderh., vol. xli.

often as the acquired form; erythematous and fatty degeneration of the heart and of the large arteries are exceedingly rare; brown atrophy of the heart is uncommon; the coronary arteries are normal; the myocardium, with the exception of thin deposits found on circumscribed parts of the pericardium, mainly near the insertion of the inferior cava and on the apex, does not suffer until some infectious disease has had an opportunity to affect it. After all, it appears fair to assume that the appearance of murmurs, no matter of what name, requires the presence of some changes in the cardiac structure which in most cases should be ascertainable.

Murmurs are caused by congenital heart diseases, which are frequent. Generally these prove fatal within a few years, with the exception of defects in the ventricular septum and of the occasional cases of subacute or chronic endocarditis, which are sometimes met with in pale and puny children without a history of traceable causes, that may have been unrecognized rheumatism or some other infectious disease. That is the more probable the less the symptoms of rheumatism are pronounced in the young. As early as 1875[1] I could point out what has been confirmed since, that local pain and swelling, even fever, may be less marked in the rheumatism of the young than in that of the adult, and still endocarditis is more sure to come and more frequent—indeed, sometimes the first and almost only symptom. Besides, rheumatism, whether ushered in by pharyngeal infection or otherwise, is more liable to be monarticular in children than it is in adults, and, therefore, liable to be overlooked. That is why many a case of rheumatic arthritis has been taken for traumatic, and *vice versa*. A girl of seven years entered my ward in Roosevelt Hospital with an old double, very coarse, and hard mitral murmur and a painful left shoulder; had a new attack of endocarditis, followed by pericarditis and pneumonia in the two lower lobes; was still kept in bed, when four weeks afterward the left shoulder was taken, and had not left the ward

[1] Seguin's Lectures, vol. i. No. 11.

when, three weeks after, the left foot was attacked with a new endocarditis. In the intervals she was free of pain and fever for weeks in succession. Thus three successive attacks of rheumatism, between which she appeared to be rapidly recovering, were monarticular.

What I mean to emphasize is this, that the absence of a history of rheumatism or some other infectious disease does not prove the non-existence of the latter. The variability of the symptoms, the difficulty of diagnosis, the absence of intelligence or experience in the parents, are just as many impediments to the correct appreciation of the present murmur. A systolic mitral murmur in a small child may easily be taken for functional when there is no history to explain it. The fact is, however, that while "rheumatism" is vehemently denied, "growing pains" are admitted, either with equanimity or with pride.

Murmurs are often, probably mostly, occasioned by an uneven pathological endocardial surface or by the incompetency of a valve. This incompetency may result from structural change or from faulty innervation. Bloodvessels, also, in order to facilitate the production of a murmur, should have an uneven surface; mere narrowness does not cause it; for in several cases of congenital chlorosis in girls with narrow, but probably smooth, arteries, I never found a murmur, nor was there one in the case of a baby, fourteen days old, with narrow arteries, that was described by A. J. C. Skeene in the *American Journal of Obstetrics*, 1876. In two others, five and seven years old, I did find in the aorta and carotids murmurs which persisted as long as the children were under observation, without other anomalies, and without an opportunity to make a thorough diagnosis.

The occurrence of cardiac murmurs in abnormal conditions of the blood appears to admit of no doubt in adults, no matter whether they are due to them alone or to structural or functional changes in the heart and bloodvessels. The latter—*i. e.*, functional changes—should be doubted as long as the altered function may be explained by the altered structure. In chlo-

rosis, scurvy, hæmophilia, leucocythæmia, and pernicious anæmia of the adult a murmur is seldom absent; in the same conditions of the child, particularly of the infant and young child, it is rarely present. When we remember the superior development and the undisturbed condition of the young heart we feel obliged to attribute the absence and the presence of the murmurs, as the case may be, to the condition of the heart and not to that of the blood. Thus we should not be too anxious to claim any of the murmurs observed in the above-mentioned conditions as "functional," to the absolute exclusion of an organic origin. Similarly do we find that chorea, for instance, in the adolescent and adult that had many years to develop the rheumatic or other infectious form of valvular disease, is almost always attended with a murmur, while there are many cases of the same complex of symptoms in the young not so attended. It is only in those cases in which a murmur appears and rapidly disappears, and is again observed after an intermission of hours or days, that we are fully justified in believing it to be functional only; for though it be the result of incompetence, either of muscular strength or of innervation, the structural changes, if any there be, cannot be very radical. Such differences are, to mention another instance, exhibited in bad cases of masturbation, which when excessive will cause a cardiac murmur in the adolescent or adult, but never once in the forty years during which I observed many hundreds of cases in the very young. Another instance is that of rapid growth, which, in the very young, does not result in a sufficient disproportion between the heart and the body to cause a murmur, while adolescents mostly exhibit it as a symptom of cardiac incompetency.

In all these cases it appears that it is the condition of the heart which causes the murmur, but neither the blood nor an abnormal process of general nutrition or development.

In chlorosis the sounds of the heart are variously changed; there is frequently a systolic and now and then a diastolic mitral murmur, not always persistent; the second sound is duplicated in bad cases only. Nor is the presence of fully

developed chlorosis required to yield these alterations; neurasthenic young women may exhibit the same changes; they are also found in some cases of lead disease. In most instances the patients are pale and nervous, the pulse is small; nose-bleeding may be frequent, but dropsy there is none. It appears, therefore, that nervous influences alone are sufficient to cause temporary murmurs. Indeed, there are those who retain the existence of a spastic contraction of the mitral orificial ring without any organic alteration. According to H. Audeoud and Ch. Jacob-Descombes,[1] Revilliod teaches that there is a temporary mitral and orificial narrowness. Constantin Paul describes a murmur, either soft or rough, over the pulmonary artery, complicated sometimes with a jugular and mitral murmur, which he attributes to the co-operation of anæmia and of spasm. Huchard assumes the presence of a spasm of the coronary artery with symptoms of angina pectoris; he also speaks of "pseudo-angina" in nicotinized neurasthenics. Foville discriminates two kinds of spasm, one of which terminates in palpitations, the other in syncope. In all their cases and those of a few others there were some symptoms referable to the alleged changes; in some there was an increased area of dulness; in others there were either pulmonary, or mitral, or vascular murmurs, which would not persist, but disappear and return.

Dombrowski speaks of an organic and of a functional insufficiency and stenosis of the aorta and of the tricuspid. Drasche, Heitler, and Dombrowski report cases of functional mitral insufficiency caused by feebleness of the myocardium. Among others, Austin Flint described long ago cases of presystolic murmurs referable to mitral stenosis, when there was much aortic insufficiency, but at the autopsies nothing mitral. Thus both spasm and incompetency are believed to cause murmurs. Both may be explained by insufficient innervation, and murmurs thus produced deserve to be called functional as long as the anatomical condition of the nerves whose

[1] Les altérations anatomiques et les troubles fonctionelles du myocarde, 1894, p. 119.

physiological action is at fault cannot be calculated or even estimated. The complexity of the anatomy and physiology of the centres in the medulla and of the peripheral nerve branches is so great as to render every attempt at exactly weighing their abnormal action perfectly futile.

Leaving the field of neurology we are on much safer ground when considering the normal and abnormal action of the heart, mainly in regard to the origin of murmurs, in connection with the condition of the heart muscle, which has not had the attention it deserved bestowed upon it until a few years ago.

Endocarditis and pericarditis have until a short time ago attracted the principal attention of clinicians. Indeed, the lesions of the myocardium were extensively studied by pathologists before their results were utilized in the interest of diagnosis and practice. And still there is no period of life in which the muscle of the heart may not be of pathological interest. Even coarse lesions may be found in early infancy, beside those congenital chronic cases of endocarditis and arrests of development which lead to cyanosis. In Gerhardt's *Handbuch*, twenty years ago, Dusch published fourteen cases of acute and seven of chronic myocarditis, and seven cases of aneurism of the heart, five of which were in the ventricular system, two in the wall, in children. Diverticula of the heart have been found even in the newly born, also intertrabecular defects; they were sometimes of syphilitic character.

Of the latter variety was that found by J. Arnold[1] in a syphilitic female one and one-half months of age. Syphilis will probably be found in many more instances of congenital heart disease than have been hitherto recorded. If so the lesions will not necessarily be confined to the right side.

In advanced age myocardial changes are frequent. Their causes are numerous; indeed, most diseases affect the heart muscle. Not to mention the results of thrombotic and embolic processes which lead to anæmic necrosis, to septic infarc-

[1] Virchow's Archives, vol. cxxxvii.

tions, or to fibrous myocarditis, we meet with acute myocarditis mostly in infectious fevers, accompanied with swelling of the intermuscular cellular tissue, minute extravasations, and sometimes fatty degeneration. The same infectious fevers, also the presence of endocarditis or pericarditis, may give rise to granular degeneration of the parenchyma of the heart muscle to such an extent as to obliterate the striated structure. This is the condition which was formerly attributed to the influence of excessive body heat only. Fatty degeneration, mostly of the left ventricle, depends on the failing nutrition of anæmia, cachexia, age, or fevers, and complicates changes in the pericardium and in the coronary arteries. Fatty overgrowth of the pericardium or between the muscular striæ is often found beyond middle life; brown atrophy, with its pigmentation mostly about the nuclei, which follows valvular disease and is met with in the senile heart; amyloid degeneration of the connective tissue; amyloid or hyaline changes in the bloodvessels, with their influence on the nutrition of the organ; and calcareous deposits, small or large—all of them are frequent, and many are found in autopsies after no symptoms pointed to their presence. Still, there are often symptoms caused by them. There may be dyspnœa or angina pectoris; the pulse may be feeble, irregular, frequent, or slow; the cardiac rhythm galloping, the sounds replaced by murmurs. Indeed, murmurs are a frequent result of myocardial changes. I have seen them coming and slowly going, when they could be explained by nothing else. That parenchymatous changes in the heart muscle, and still more that interstitial inflammations of the connective interfibrillar tissue, should get well under the influence of tonics and rest and medication, can be denied only by those who have not seen the different stages in the same processes—invasions and recoveries—in other organs. The correct estimation of myocardial changes in the living, however, is beset with peculiar difficulties, mainly in this, that they may be local and not accessible to percussion. Even thorough and universal myocardial alterations need not change the size of the organ.

A few conclusions appear to be self-evident:

1. The diagnosis of deranged function in any organ is only a makeshift, and justifiable only as long as we are ignorant of the physical cause of that derangement. A functional heart murmur is one the anatomical cause of which we do not know. That is why a skilled diagnostician may recognize fewer functional murmurs than one who will not diagnosticate a heart disease unless he have all the symptoms, including dilatation and hypertrophy.

2. The same disorders of the blood and nervous system in which heart murmurs are observed in the adult do not cause them in the small infant. In the latter the heart is larger, more massive, and more powerful, and its contractions are more uniform and effective; its two ventricles are equally muscular, and the valves are smaller. Thus the greater frequency of murmurs in the adult is attributable to the physical condition of his heart, and should not be explained by a deranged function.

3. Even in the present limitation of our knowledge we should agree to call functional only those murmurs which are temporary, or intermittent, or variable in their character. They are met with in the neurotic and neurasthenic, in the (adult) anæmic, sometimes in syncope or in chorea minor, and occasionally in rheumatism. Even here they should be recognized either as myocardial or as neurotic.

DISCUSSION.

Dr. Howard S. Anders, of Philadelphia: I think Dr. Jacobi's paper and experience certainly have a tendency to clear the atmosphere. So far as my experience goes, it has led me to the belief that the term "functional cardiac disease" can be largely done away with. Functional cardiac disease usually means relative insufficiency of one of the heart orifices; or, if it means anæmia, we certainly have no reason to call it functional. If, as often happens, the so-called functional murmur of relative insufficiency is due to myocardial disease, or if there is an alteration in the constituents of the blood, the mur-

mur is certainly not functional but organic. It is more accurate to call these diseases organic.

Dr. E. Fletcher Ingals, of Chicago: I have been impressed with the statement made by Dr. Jaeobi that functional disease of the heart is far rarer than we suppose. This belief has been growing upon me of late, and I have become convinced that either I made some mistakes in diagnosis in past years, or else that functional cardiac disease is less frequent now than formerly. I have under my observation at present a patient whom I first saw ten or twelve years ago with what I then regarded as a functional murmur. There is now no question of its organic character. I believe that many of our so-called alcohol hearts, or tobacco hearts, or coffee hearts, if followed up long enough, will prove to be organic rather than functional.

Dr. John C. Munro, of Boston: I hesitate to say anything in contradiction to what Dr. Jaeobi said, but, three years ago, in my surgical wards at the Boston City Hospital, I had the patients examined very carefully immediately preceding and following operation, regardless of the nature of the operation. Upon summing up our results, we found that a fair proportion of the cases—I cannot remember the exact figures—had a marked functional murmur (so called) at the apex and base, with an accentuated pulmonic second sound; during the period just preceding the operation, but within two or three days after the operation, the murmurs disappeared. This was especially noted in women of the shop-girl class, in whom a murmur would rapidly appear when the operation was imminent, and disappear as rapidly if the operation was postponed or after its performance.

Dr. Babcock: Was there always tachycardia at those times?

Dr. Munro: I do not know.

Dr. Harold Williams: It is customary to speak of these so-called functional murmurs as hæmic and dynamic. There is no sufficient reason to believe that hæmic murmurs really exist. Dynamic murmurs are, literally speaking, organic. This entire subject has excited much discussion. Dr. Prince, of Boston, examined many applicants for appointment as firemen and policemen, and he found that those who were very apprehensive suffered from a tumultuous action of the heart and had systolic murmurs which corresponded with all the requirements of mitral regurgitant murmurs. They were functional murmurs, due to interference with the function of the heart, and rapidly disappearing. He accounted for them by over-contraction of the left ventricle forcing the muscular mitral valve.

I have been impressed by the presence of these functional murmurs in overtaxed hearts where the exact converse existed to what Dr. Prince found—namely, a too feeble contraction of the mitral valve.

This too feeble contraction causes incomplete closure of the valve cusps, and is, consequently, followed by mitral regurgitation. Thus, it would seem that the so-called functional murmurs are really organic, in that they are due to a temporary derangement of the organ. In the same way "organic murmurs" are functional, in that their origin is a functional disturbance due to organic defect. It is, therefore, a question of nomenclature; but so long as the real meaning of the words is clearly understood, it would seem to me better to retain the expression "functional murmurs," than to run the risk of further obscuring the subject by classing them as organic. It is really only a question of words.

Dr. Jacobi: There undoubtedly are functional murmurs, and those alluded to by one of the speakers as occurring before operation, and some of the others mentioned, are certainly of that nature. There are functional murmurs of other kinds. For instance, now and then in severe cases of chorea you will get a heart murmur for a day or two, which then disappers, only, perhaps, to appear and disappear again later, and such a changeable murmur is certainly functional. The fact should be borne in mind that murmurs produced in veins have been mistaken for heart murmurs. West, eminent though he was, frequently made that mistake.

As far as so-called hæmic murmurs are concerned, which are attributed directly to anæmia, I do not believe that they exist. Even in pernicious anæmia or in infantile scurvy no such murmur may exist. This would prove that anæmia by itself does not produce such a murmur, but when the anæmia is of long standing and has affected the structure of the tissues, we may have a murmur which is the result of malnutrition of the tissues of the heart and of the blood-vessels. In accordance with that we cannot expect to find a hemorrhage from the bloodvessels, no matter what the condition of the blood may be, unless there is an abnormal condition of the vessels themselves. When the bloodvessels become under-nourished by the condition of the blood, then there will be a hemorrhage. Something similar, I think, is the relation between anæmia and functional heart murmurs.

BICYCLING IN ITS RELATION TO HEART DISEASE.

By A. C. GETCHELL, M.D.,
WORCESTER, MASS.

Synopsis:

1. The manner in which excessive bicycling may produce dilatation of the heart, either temporary or permanent, also hypertrophy of the heart and disease of the valves and great bloodvessels.
2. The application of these principles to—
   (*a*) Young children.
   (*b*) Young adults.
   (*c*) The middle aged and the unhealthy.

It has been shown, particularly by Allbutt[1] and Da Costa,[2] that continued work at laborious avocations and continuous activity of a milder sort, such as long marches of soldiers, may produce not only functional disorder of the heart but actual disease of the heart and bloodvessels as well.

I propose in this paper to discuss the effect of another kind of continued muscular exertion upon the heart—namely, bicycling. In the consideration of the subject I shall divide the riders into two classes, the untrained and the trained.

First the untrained. Under this class I include those who are learning to ride and those who ride for pleasure or business only, in contradistinction to the athletic amateur or professional rider.

The accident that these riders are most liable to is dilatation of the heart, and particularly of the right heart, which may be permanent or may last for but a short time. Acute dilatation happens in this wise. Under the stress of prolonged and severe muscular activity the demand for pure blood in the tissues is greatly increased. This is met at first by more

rapid breathing and aeeelerated heart action. But if the demand be too long continued, or through special causes, like rapid hill climbing, be too greatly increased, the right heart cannot send the blood rapidly enough through the lungs. Some remains in the right ventriele at the end of each contraction. This amount increases with eaeh eardial eyele, and the cavity is stretched. The rider is now out of breath. If the task he has set out to accomplish is too great, and he persists, the dilated ventricle fails to aet, and uneonseiousness ensues. Through the cessation of bodily action the heart is relieved of its burden and resumes its funetion if it have suffieient vitality. On the other hand, if it be diseased, death may ensue.

The following cases are examples of this accident, and they illustrate different conditions under which it may happen:

Herschell[3] was riding with a friend who was new to the wheel. After riding up a long hill he noticed that his eompanion was much exhausted, that his faee was pale, and his lips blue. He was obliged to dismount and lie upon his back on the grass. Examination showed marked dilatation of the right heart, feeble impulse, and rapid pulse. After an hour he was able to resume his journey, and in the evening the heart was normal.

A patient of mine had a similar experienee. He was riding with his sister, and at the summit of a long and gradual ascent reached out to help his companion by pushing her over the erest, when he fell from his wheel uneonscious. He soon recovered, and after a time proceeded on his ride.

This man was about thirty-five years of age, and had ridden the bicycle for several years. Of late years his health had been very good, though he suffered somewhat from dyspepsia. As a child, however, he was delicate. His father died of heart disease. I have never had an opportunity to make a satisfactory examination of his heart. Once, however, I had occasion to spray a 4 per cent. solution of cocaine into his nose for the purpose of an examination. Pronounced and distressing eardiac symptoms immediately ensued, which

lasted to a greater or less degree for several hours. At this time I made a superficial examination, but detected no lesion.

A retired wine merchant, while riding over a level road at a moderate pace, fell from his wheel and soon died. Autopsy showed rupture of a heart diseased with fatty degeneration, and also a stomach full of an undigested meal.[3]

These are instances of acute dilatation occurring, first, in healthy persons who are subjecting themselves to severe exercise when, as the horsemen would say, they are "soft;" secondly, of those who are inured to the exercise, but who have weak hearts; and, thirdly, of those whose hearts are actually diseased.

It is well recognized that dilatation of the heart occurs in the course of severe illness, and particularly when there is marked febrile movement. Under these conditions there are two factors to be considered in its production: first, the integrity of the heart muscle, and, second, the resistance to be overcome. In disease the nutrition of the heart may suffer more or less according to the amount of toxic products which are circulating in the blood. There may be obstacles in the blood-current, as, for instance, a consolidated lung.

The problem is not so clear in the question of dilatation from severe muscular exertion in the well. At first sight it would seem that neither of these factors is present, because physiologically increased activity of the body in the open air would seem to purify the blood, and increased muscular exertion would send it more freely through the tissues. This is true up to a certain limit, and that limit depends upon the relation of two factors—the amount of work to be done and the ability of the organism to do it. To quote Gibson:[4] "There is one consideration (in the production of dilatation) that must not be overlooked. While the effect of stimulating a muscle in experimental physiology is to accelerate the flow of blood through it, the effect of long-continued muscular action must be to form a large number of waste products, and it will necessarily follow that the blood may have greater difficulty in passing through the tissues after it has been

loaded with such substances. The mere muscular exertion, moreover, involved in long-continued efforts is to increase intrathoracic pressure, and this added to the greater impurity of the blood will interfere with the functions of the right side of the heart."

In bicycling a proof of the existence of toxic matters in the blood is found in the so-called fatigue fever, which is produced by the absorption of toxins into the blood. In its milder forms it is not uncommon. A personal experience furnishes a good example: One hot day last August I rode with a party over a hilly road to the foot of a small mountain, ascended the mountain, and returned home. The trip occupied the entire day, from 9 A.M to 5 P.M., with a short rest at the top of the mountain. I was on a tandem bicycle, accompanied on the ride out by a child of ten years and on the return by a lady. I was not particularly tired on my return. During the night, however, I became very restless and could not sleep. My pulse was rapid, though regular, and I felt very hot. The experience was so novel, and to me then unaccountable, that I could explain it only on the supposition that I was threatened with some acute illness. After a very uncomfortable night and an unsuccessful attempt to breakfast I remained in bed during the entire day. I was not a novice at the wheel, having ridden a good deal for ten years. In some instances there is much more cardiac disturbance, and an irritable condition of the heart persists for several days.

The second factor in the production of permanent dilatation of the heart is increased peripheral resistance. This ensues in part as a consequence of the accumulation of toxic products in the blood and in part in the stasis that occurs in the lung owing to the inadequate action of the right heart. Add to these causes the occasional severe strain of spurting and rapid hill climbing and the crouching attitude of the scorcher, and we have causes adequate for much trouble. It thus appears that bicycling may furnish the two conditions for the production of permanent dilatation of the heart—namely, toxins in the blood and increased peripheral resistance.

Dilatation is not the only evil to be feared. Valvular disease may result as well.

The auriculo-ventricular orifices undergo changes in dilatation of the cavities, and thus their dimensions are considerably increased.[4] As a consequence of this the cups of the valves do not meet as perfectly as in health. In acute illness a murmur which later disappears may be heard at these orifices. Under these conditions both the muscular walls and the auriculo-ventricular ring have not stretched beyond the limits of a possible return to the normal condition. Even if a ventricle weakened by disease has become dilated it is possible by rest and proper treatment to bring about either a restoration to the normal condition or a compensatory hypertrophy. It is doubtful if such a process is possible to an overdistended auriculo-ventricular ring, and as the cusps remain the same size a permanent disability of the valve must follow.

Let us now consider the effect of the exercise upon the trained rider. The claim is put forth that there can be no danger to men, and, indeed, women, of this class, since they never are distressed by their exertions. But an examination into the conditions shows that this reasoning does not hold good.

An hypertrophy of the heart as the result of continued and severe muscular effort is regarded as physiological. Thus the hearts of race-horses and greyhounds are found to be greatly hypertrophied. Moreover, teachers of gymnastics tell us that the best results in the development of the peripheral muscles are attained by continued exercise against slight resistance rather than vigorous action against great resistance. According to Gibson,[4] a prominent feature in the production of pathological hypertrophy of the heart is its rapid action. Da Costa[2] found hypertrophy to result from the irritable condition which he described in soldiers. Osler[5] says: "The condition (*i. e.*, irritable heart) is not infrequent in civil life among young men, and it leads in some cases to hypertrophy of the heart." And Hershell[3] is of the opinion that hypertrophy may result from long-continued palpitation.

We have seen that one of the characteristic features of fatigue fever from bicycling is long-continued acceleration of the pulse. This acceleration also occurs without any fever or uncomfortable sensations.

It has been found by a number of experiments with different classes of riders, men, boys, and girls, riding over roads of different grades and against head winds, that after riding at a certain rate, the pulse being counted, if the speed be increased the pulse-rate increases in greater ratio, and that, too, with the rider feeling no sense of fatigue. Thus the pulse-rate was found to increase from a normal of 72, 74, 80, and 84 to 112, 130, 140, and in some instances this increased rate was maintained for several hours.[3]

Riders of this class do not suffer from the accumulation of toxic substances in the blood as do their less vigorous companions, and the blood is sent through the tissues with greater freedom. But still there is a certain amount of resistance to be overcome, and when we add to these two factors—namely, long-continued rapid movement and slight peripheral resistance—a third, good nutrition, which is the result of bodily well-being produced by training in the open air, it is readily seen that we have the conditions most favorable to the production of cardiac hypertrophy.

This does not mean that all trained bicycle riders suffer from hypertrophied hearts, medically speaking, because their training and life enable them to maintain the nutrition of the heart as well as that of the body at such a state as to fully compensate any alteration of structure.

It is said, however, that athletes are liable to rapid deterioration of health when they relinquish their active exercises.[6]

What is the effect of such over-exertion on the bloodvessels? As Allbutt[1] has well said, the muscular ventricle may grow both in size of its cavity and in the thickness of its walls, but the walls of the aorta suffer under greater limitations; its power of resistance is great, but its activity is nothing more than the recoil of its elastic fibres.

Such repeated stretching results not in increased strength

but rather in weakness, as one of the coats may give way and pouching result, or an endarteritis ensue, or, indeed, incompetence of the aortic valves follow.

Lannois[7] reports a case of rupture of one of the aortic cusps. It occurred in a man of thirty-eight years, during a ride from Paris to Madrid, a distance of 1452 kilometres, in nine days. There was not at any time pain or other evidence of the accident, nor was his health impaired, and the lesion was discovered some time later rather by accident.

To summarize: Acute dilatation of the heart is an accident that may be expected from over-exertion in an unathletic rider, and if the conditions be repeated, permanent dilatation, especially of the right heart, may result, with a consequent damage to the auriculo-ventricular valves. And, secondly, in the athletic hypertrophy may be produced, with a possible consequence of disease of the aorta and incompetence of the aortic valve.

In discussing the application of these principles I shall consider three classes of riders:

(*a*) Young children.

(*b*) Young adults.

(*c*) Persons approaching middle life and the unhealthy.

Fortunately, in the past the price of a bicycle has precluded its use as a mere toy, and very young riders have been comparatively few. Still one sees them not only in the parks and streets, but in the country, as well, far from home. I can see no justification whatever for this practice. Boys and girls under thirteen or fourteen years of age should not be allowed to take rides into the country where they may get far from home and be subject to unwise exertion of a long ride or immoderate hill climbing. I doubt, too, the wisdom of allowing a young child to ride *ad libitum* even in the park or on asphalted streets. The sport is fascinating and not fatiguing, and it is not an uncommon experience to see young children riding hour after hour. Even on a level space, as we have seen, and entirely within the range of non-fatigue, the heart's action may be accelerated, and this increased

activity may persist long after the riding is over for the day. Nor does the child escape more serious consequenees, as this incident, which oceurred in my neighborhood, shows:

The child was a girl of twelve years, previously fairly healthy. It had been noticed, however, that she got out of breath easily while riding. One afternoon she went out into the eountry, which was rather hilly, with friends for a ride. Nothing unusual was notieed except that she was somewhat out of breath going up hills. She took tea with a friend, and after the meal again rode about the square, this plaee being slightly hilly also. On going to bed she had some gastric distress, and the family physieian was called. He regarded the symptoms as due to indigestion and prescribed accordingly. The child's room was adjoining her mother's, who was wakened in the night by her daughter's distressed breathing. Before help could be called the child died. There was no autopsy, but it is apparent that death was due to aeute dilatation of an unhealthy heart pressed upon by a distended stomach.

In the second class of cases, healthy young adults, there is need of precaution against excessive hill climbing and very rapid riding, as the following cases show:

C. F., aged twenty years, employed in a sewing machine factory, consulted me for relief of a frothy expeetoration. His family and personal history was good. He had been a bicycle athlete and had ridden in many raees. His early symptoms were eough, a feeling of snbsternal oppression, and uncomfortable sensations referred to the eardiac area. He stopped riding and grew better, to resume it after a winter, only to be obliged to discontinue it altogether. His throat and lungs were without evidence of disease. The impulse of the heart, which was feeble, was felt in the sixth interspace, just inside the nipple line. The sounds were fairly pure in charaeter, but faint over the trienspid area, while over the aortie and pulmonie areas the first sound could not be heard at all. In this instance there was dilatation following hypertrophy.

In the following case the opposite conditions prevailed, first dilatation then hypertrophy: H. B., aged twenty-six years. Before the age of twenty years he had always been well. Then he began to ride the bicycle. He lived in a very hilly town, was a scorcher, rode a wheel with 80 gear, and never got off for a hill. Presently he found himself unable to ride, and for some time after giving up the wheel was not in good health, having frequent fainting spells. He gradually grew better and resumed riding now in moderation. His heart presented these features when I examined him five years after his illness: The apex beat was apparently to the right of and below the ensiform appendix, really the impulse of an hypertrophied right ventricle. There was decided pulsation over the lower right portion of the sternum. Dulness extended one and a half inches to the right of the median line and three inches to the left at the level of the fourth rib. The first sound over the epigastrium was impure. The pulse was 120 and regular. The rapid pulse was due to the excitement of the examination. He said it was usually 72. But this acceleration showed that the heart was irritable.

In this connection it would be interesting to learn the experience of the surgeons who examined recruits for the army in the late war. I have been able to find but one reference, however.

In an article in the *Medical Standard*, detailing the results of the physical examination of 9901 officers and men of the Illinois National Guard, Lieutenant-Colonel Charles Adams, speaking of the heart, says: "In the preliminary examination of recruits made by medical officers of different regiments before leaving their home stations, many cases of bicycle heart were discovered and not allowed to enlist. These were characterized by hypertrophy and dislocation of the apex beat, with irregular or rapid beat."

Da Costa's[2] tables show that fully two-thirds of his cases happened from the sixteenth to the twenty-fifth year, while the great percentage was from the twentieth to the twenty-fifth year. These figures are fairly reliable for our present pur-

poses, because our army was recruited from men of the same class as our bicycle army. Men, then, and women also, who take to the wheel between the ages of twenty-five and thirty-five years, if healthy, are little liable to trouble, because their physical powers are matured, and also because, as a class, they are not apt to indulge in the excesses of the more youthful.

From the age of forty years, however, conditions are likely to be different. Those who begin to ride at this time in their life are the man of leisure or the bookkeeper or clerk, people who have spent the greater part of their life indoors, have very probably been high livers, and, as a consequence, suffer from faulty nutrition. To such the bicycle may be a blessing or a positive danger. Proper regard for hills, excessive exercise against high winds, rough roads, and the gear of the wheel will make the exercise beneficial, while one indiscreet over-exertion may cause irreparable injury.

And, lastly, those who seek medical advice for ill-health, such as dyspepsia. In all such cases, before advising the bicycle, the heart should be carefully examined, for it must be remembered that the symptoms for which the patient seeks relief may derive their origin from some disorder of the heart or bloodvessels. Indeed, instances are on record of persons by bicycling at first getting much relief from their dyspeptic or gouty symptoms, only later to fall victims to fatal disease of the heart.[3]

Under these conditions an examination of the heart should be exhaustive by the use of every means at our disposal—palpation, percussion, mensuration, and auscultation. The slightest deviation from the normal condition, whether it be in the character of the sounds, the relative intensity of the first and second sound in any area, their relative intensity in different areas, the rhythm of the heart, the character of the impulse, the condition of the arteries, should be carefully noted, for it may be that the heart that presents no gross lesion is the dangerous one, and the one that soonest breaks down under a severe strain.

Even if cardiac or circulatory lesions are found it does not

follow that the bicycle should be forbidden, but it must be prescribed. Definite regulations must be laid down as to the character of the road, the length of periods of exercise, the kind of wheel, and the size of the gear. From personal experience I feel sure that proper bicycle riding will materially strengthen a weak heart, as it undoubtedly improves the general nutrition of the body. My contention is not that the bicycle is a harmful mode of exercise. Indeed, I hold to the opposite view very strongly. Bicycling is productive of great good, both in the way of health and of pleasure. But at the same time we must recognize the fact that it presents peculiar temptations to excessive exertion that is dangerous.

The athlete will probably go his way without asking or taking our advice. But we should be in a position to influence the youth in our midst, and especially those who seek medical advice. If the bicycle is prescribed as a therapeutic agent for any trouble, including disease of the heart, it must be carefully prescribed and not merely recommended, leaving the patient to his own inclinations and possible indiscretions.

References.

1. Allhutt. The Effects of Overwork and Strain on the Heart and Great Blood-vessels. St. George's Hospital Reports, vol. v. p. xxiii.
2. Da Costa. On Irritable Heart. American Journal of the Medical Sciences, N. S., vol. lxi. p. 17.
3. Herschell. Cycling as a Cause of Heart Disease. London, 1898.
4. Gibson. Diseases of the Heart and Aorta. 1898.
5. Osler. Practice of Medicine. 1892
6. Herschell. Cycling as a Cause of Heart Disease. Lancet, 1895, vol. i. p. 541.
7. Lannois. Insuffisance aortique par rupture valvulaire chez un bicycliste. Bull. et mem. Soc. Med. d'Hôp. de Paris, 1896, 3 S. xiii. 852, 853.
8. Medical Record, vol. liv. p. 274.

THE EFFECTS OF VIOLENT AND PROLONGED MUSCULAR EXERCISE UPON THE HEART.

By HAROLD WILLIAMS, M.D.,
BOSTON, MASS.,

AND

HORACE D. ARNOLD, M.D.,
BOSTON, MASS.,

THE following observations are based upon an examination of the contestants in the recent Marathon race, given under the auspices of the Boston Athletic Association, April 19, 1899.

Our thanks are due to the governing committee of the association and to Mr. John Graham, its athletic manager, without whose assistance the investigation would have been impossible; also to the athletes themselves, who, with one or two exceptions, lent their cordial co-operation.

A Marathon race is a road race, corresponding in distance and conditions to the course from Marathon to Athens. It was instituted at the ancient Olympic games in commemoration of the performance of the messenger who ran from Marathon to Athens after the great battle, to announce the victory of the Greeks. This race was revived at the recent Olympic games in 1896, and the course was laid out from the town of Marathon to the Stadium in Athens.

Since 1896 an annual Marathon race has been given by the Boston Athletic Association. The course extends from the town of Ashland to the club house in Boston, a distance of twenty-five miles, over smooth roads, up and down several hills, many of which are both steep and long.

The conditions are such that the contestant must cover the distance without the aid of other locomotive power than his own. He can walk or stop if he so elects, but he can receive no locomotive assistance. The winner is the contestant who performs the distance in the shortest time. Each contestant, in case of accident, is accompanied by a member of the ambulance corps of one of the militia regiments, mounted on a bicycle. In relief of possible apprehension, I would say in advance that no accident occurred in the present race except to a member of the ambulance corps, whose bicycle collided with a dog.

The weather on the day of the race was pleasant, but an easterly wind, which was a head wind, greatly impeded the runners. There was considerable dust. The mean temperature of the air was 44° F.

The time made by the winner was 2 hours 54 minutes 38 seconds, about a minute less than the Athenian record, but twelve minutes more than the Boston Athletic Association record of last year. The time of the winner thus averages a little better than a mile in seven minutes for the twenty-five miles, from which it can be seen that he did not avail himself of the privilege of walking or resting to any considerable extent.

The physicians associated with me in the examination were Drs. H. S. Dearing and H. W. White, of St. Elizabeth Hospital, and Dr. E. C. Stowell, physician to the Boston Dispensary, all of Boston, and all members of the Department of Clinical Medicine in the Tufts College Medical School. We are indebted to Drs. A. E. Austin and L. H. Coriat for the urinary analyses, and to Dr. Batchelder for the examinations of the blood.

Preliminary Examinations. Prior to the race preliminary examinations were made upon the athletes in training, before and after long practice-runs. The results of these examinations, apart from their enabling us to arrange a programme of procedure to be adopted on the day of the race, were interesting in themselves.

Blood-counts were made of two runners before and after long practice-runs, with the following results:

Blood-counts.

No. of Runner.	Distance.	Before.	After.	Remarks.
1	10 miles.	6,700,000	6,000,000	Peripheral anæmia.
4	5 "	5,050,000	5,716,000	Peripheral hyperæmia.

The temperature of No. 1, before the run, was 97.5° F., and after the run, 97°. From these combined observations it was concluded that the athlete was approaching the condition technically known as "staleness"—*i. e.*, a condition of reduced bodily vigor, and the man was advised to temporarily refrain from further practice-running, and to partake of more carbohydrates in his food (his diet having been largely albuminous), with the result that on the day of the race his initial temperature was 98.2°, and that he was one of the winners of the race.

This fact goes to show the value of medical supervision in cases of this nature, as it is safe to say, in view of the facts to be described, that if this contestant had entered the race with a temperature of 97° he would have been unable to complete the distance in the time accomplished.

As a result of these preliminary examinations a uniform method of procedure was decided upon. It was further decided that the blood-counts did not justify the labor and difficulties involved. They were accordingly abandoned.

The Race. In the race itself there were seventeen entries; fourteen completed the distance. Notes were taken of thirteen. The contestants were examined in Ashland during the hour and a half preceding the race, and in Boston during the first few minutes after the race. The history was taken of eleven who finished. The oldest was twenty-eight years, the youngest nineteen years; average age, twenty-two years. The parents of all were living, with a single exception. The occupations of the contestants were: clerks, 4; plumbers,

2; blacksmith, 1; oiler, 1; printer, 1; steward, 1; cigar-maker, 1. Four had had scarlet fever; one typhoid; none rheumatism. Nine drank tea; 5 coffee. Three use tobacco (one a cigarette-smoker). None use alcohol. Eight were of neurotic temperament. Three had competed in Marathon races before. For two men it was their third competition.

Training. The training was noted in 14 contestants: maximum, 7 months; minimum, 2 weeks; average duration, 2 months. With the single exception of an athlete who trained four and a half weeks, it may be said that those who trained under two months were least successful in the race. The training in all cases consisted of a mixed diet, with increased albuminoids and decreased carbohydrates, and practice-runs three or more times a week. One of the men was taking strychnine; one ate apples on the way. One man who had been training but two weeks dropped out.

Physical Examination. *General inspection* was altogether favorable. Each contestant appeared a healthy, vigorous young man. After the race the general inspection showed a greater or less exhaustion in every case. Three were dazed and confused; 1 cyanotic; 1 of an ashen color. Three had to be carried to the examining-room.

The Pulse. The radial pulse was taken before and after the race in 10 individuals; after the race in 13:

Pulse.

	Number of athlete.[1]												
	1	2	3	4	5	6	7	8	9	10	11	12	13
Before	72	...	...	70	80	72	72	78	85	88	...	80	88
After	120	120	104	108	120	112	120	70	60	112	112	106	108

Nos. 8 and 9, showing the lowest pulses, were the most exhausted men; in No. 8 it was intermittent, in No. 9 irregular.

[1] The numbers of athletes in these tables are not the race numbers, but are arbitrary numbers used instead of names to distinguish the athletes. In all the tables these numbers correspond.

Before the race the pulse was strong in all. After, it was very weak in all; in one the radial could not be felt for several minutes after the finish; in one it was intermittent.

TEMPERATURE. The temperature was taken in the mouth before and after the race in 11 cases; after, alone, in 3.

The temperatures in the following table marked 94—° show that they were below 94°. The thermometer employed (registered Yale) only registered temperatures above 94°. This complication of extremely low temperatures was not foreseen, otherwise another would have been provided. It would be of great interest to see how low it might go in these similar cases:

	Number of contestant.												
	1	2	3	4	5	6	7	8	9	10	11	12	13
Before	98.°2	...°	...°	99.°4	99.°6	98°	99.°4	99.°4	99.°6	100°	...°	99.°8	100°
After	94—	97	94.6	95.5	94	94—	97—	94	94—	95	94	97	98

RESPIRATIONS. The respirations were noted in eleven cases:

RESPIRATIONS.

	Number of contestant.										
	1	2	3	4	5	6	7	8	9	10	11
Before . . .	20	...	...	24	24	24	26	20	24	24	
After . . .	26	40	34	24	36	32	40	36	28	32	30

The weight was noted in ten cases:

WEIGHTS.

	Number of contestant.												
	1	2	3	4	5	6	7-8	9	10	11	12	13	14
Before .	160½	...	...	117	125	126	143	115	139	...	116	133½	134
After . .	155	...	...	112½	122	122	139¾	112½	135½	...	114¾	132	127½
Loss . .	5½	...	...	4½	3	4	3¼	2½	3½	...	1¼	1½	6½

URINE was examined before and after in 6 cases; after, alone, in 1. It showed the presence of albumin and casts in every case after the race:

URINE.

	Number.						
	4	5	7	8	9	10	13
Before	Albumin trace; occasional blood-corpuscles.	Normal.	Normal.	Large trace.	Normal.	Trace; blood-corpuscles.	
After	Albumin, $\frac{1}{10}$ per cent.; casts.	Albumin, a trace; casts	Trace; numerous casts.	Albumin, $\frac{2}{5}$ per ct.; casts.	A trace; casts.	Same; with numerous casts.	A trace; casts.

EXAMINATION OF THE HEART. Inspection of the chest before the race was made in thirteen cases. The points looked for included bulging of the chest-wall, abnormal pulsations, right ventricular heaving, retraction of the intercostal spaces, carotid and jugular pulsation, and the position and diffusion of the apex-beat.

Bulging was noted in nine cases, and consisted in a relative projection of the left costal cartilages of the fourth and fifth ribs as compared with the right. Other abnormalities were absent, with the exception of slight visible pulsation of the carotids behind the angle of the jaw in two cases and slight epigastric pulsation in one. In both cases these points were attributed to thinness and excitement. Inspection of the chest after the race was made in 11 cases. Carotid pulsation was noted in 8; jugular pulsation in 1; diffuse epigastric in 1.

PALPATION. Palpation before the race was noted in 9 cases. The force of the apex-beat was described as thrusting in 7; normal in 1; diminished in 1. The force of the impulse projected the left costal cartilage in 1. There were no thrills. After the race the apex-beat was noted in 5 cases: thrusting in 1, ticking in 2, and weak in 2.

PERCUSSION. By percussion we attempted to show the area of relative cardiac dulness, first as compared with the

normal chest, and second in order to compare this relative area in each individual before and after the race. The freedom of the chest-walls from adipose tissue rendered this examination easier and more exact than in the cases of ordinary men.

The method employed consisted in marking out the area of comparative dulness on the chest with water-color paint, after which a tracing was made upon tracing-paper. The paint was then removed, so that no lines remained on the chest to bias the judgment on the second examination. As this relative area differs so much in the percussion of individual examiners, each case was percussed in the "Preliminary Examination" by three examiners, and the results compared.

A reduced photograph of the superimposed tracings of three examiners upon one of these athletes shows how closely these observations coincided, and demonstrates the accuracy of percussion in a thin chest with an hypertrophied heart:

PLATE I.—Reduced photograph of the superimposed tracings of three examiners, showing area of comparative dulness.

This method was found to consume so much time that at the race each runner was percussed by only one examiner, in each instance the same examiner making the observation

PLATE II.—No. 12. Before and after. (Outside lines, after.)

PLATE III.—No. 5. Before and after. (Outside lines, after.)

before and after the race. In view of the fact of the close agreement in the percussion-dulness obtained by the three examiners, the constancy of enlargement is striking.

PLATE IV.—Before and after. (Left outside lines, after.)

Thirteen examinations by percussion were made before the race; 10 of these after the race. Of the 13 made before, 11 showed the area of comparative dulness relatively large as compared to the normal heart. Two showed a diminished or normal area. These two, showing the normal or diminished area, did not finish. One showed a relatively large area, but started with a temperature of 99.8°, and had a sore throat. He did not finish.

Of the ten observations made before and after the race, seven showed a relative increase of the cardiac dulness extending chiefly to the left, and corresponding to the position of the left ventricle, while two showed a decrease of the area of dulness in the same position. One showed nothing.

A comparison of the percussion-areas of one of the best with one of the poorest men, the men weighing the same, was

made. The man with the smaller area had only trained three weeks. He dropped out after running a few miles.

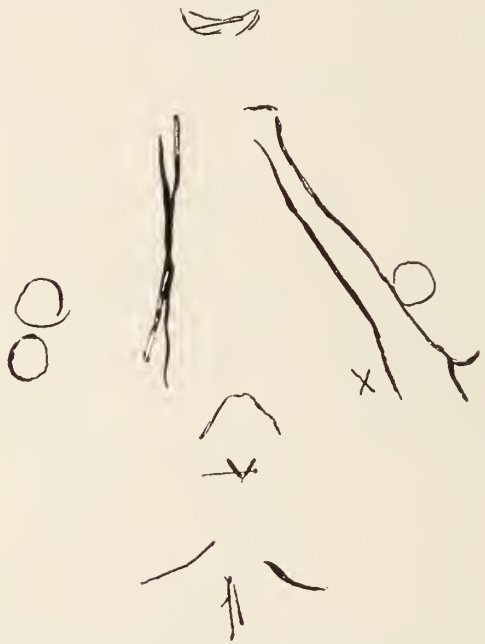


PLATE V.—Comparison of area of dulness of winner with one of the unsuccessful.

AUSCULTATION. Auscultation was practised in 11 both before and after the race; in 13 in all after the race. The heart-sounds were found normal in all examined previous to the race. They were unusually loud and full in all. As those who were not examined before the race succeeded in accomplishing a run of twenty-five miles, it would seem evident that their hearts were normal at the start, and that for the purposes of comparison the thirteen cases should be considered together.

After the race auscultation was practised with the binaural stethoscope immediately on the arrival of the men. They were brought to the examination-room in the elevator, and examined as quickly as their clothing could be removed from

the chest. The heart-sounds in all were weak. Of the 13 men so examined, in 2 there were no murmurs. In 11 cases murmurs were noted. In all cases murmurs were systolic in time, and their point of greatest intensity was at the base, slightly to the left of the sternum in the second interspace. They were in all instances transmitted downward to a greater or less extent in the line corresponding to the left border of cardiac dulness. In six cases this murmur was transmitted to the apex, in all of which it was heard in the back, between the left scapular angle and the median line. Accentuation of the pulmonic second sound was sought for, but was not distinguishable in any case, probably due to weakened action of the heart. These murmurs were ephemeral in character, and in all instances disappeared before the athletes left the examining-room.

Sphygmographic Tracings. Sphygmographic tracings were taken of 10 before and after; of 13 after the race alone. If the capability of running twenty-five miles can be regarded as proof of a normal heart at starting, these 13 tracings can be considered together. The tracings before the race, considered as a whole, showed a series of strong regular pulses of high arterial tension. This was marked in 8; less marked in 1, whose tracing shows rather a low tension than otherwise; and exceedingly low in another who had been taking strychnine.

The 13 tracings taken after the race show exceedingly low tension in 10. Four of the tracings correspond with the pulse of mitral regurgitation. In all these cases the murmur above described was heard. In one there is a suggestive mitral regurgitation-tracing, but no murmur. This man did not complete the whole distance, but came in on a car. Possibly the murmur had disappeared before he presented himself for examination one hour after he stopped running.

Summary. It is to us a source of regret that we cannot enter into the discussion of the tables in the limited time at our disposal. They are introduced here for the twofold pur-

pose of completing the report and showing the exhaustion of the men. Had the time been longer, it would have been of interest to have pointed out how greatly these results are at varianee with the conventional statements of the text-books. For example, it is stated that the temperature of the body is raised during exercise (*American Text-book of Physiology*). We found it invariably lowered, varying from 5.5° to 2°. Albumin in the urine is spoken of as physiologie—we find it invariably associated with casts, thus foreing the conelusion that casts are in some instanees physiologic, or that this albuminuria may at times exceed the physiologic limits. We are told that prolonged muscular exertion "enormously increases the blood-pressure in the arteries" (see Osler's *Practice of Medicine*, p. 736), whereas in the foregoing observations it is enormously deereased.

The results of these observations taken eolleetively show that general statements made in regard to the effeets of muscular exereise may be misleading unless the character of the exereise and the eondition of the individual are taken into eonsideration.

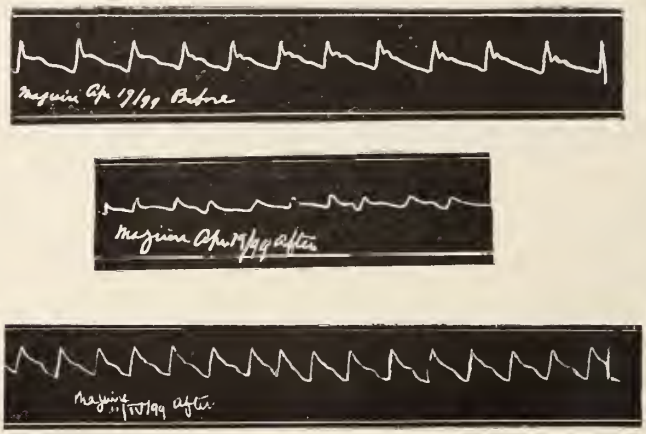


PLATE VI.—Sphygmograms of No. 4. 1. Before race. 2. After race. 3. After five miles' practice-run.

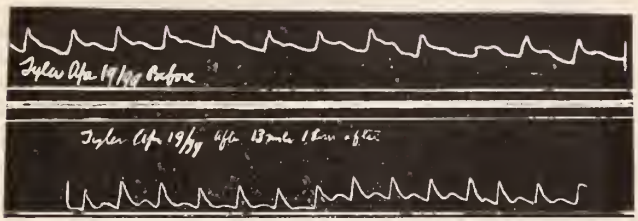


PLATE VII.—Nos 1 and 7. Dropped out. Suggestive mitral sphygmogram.

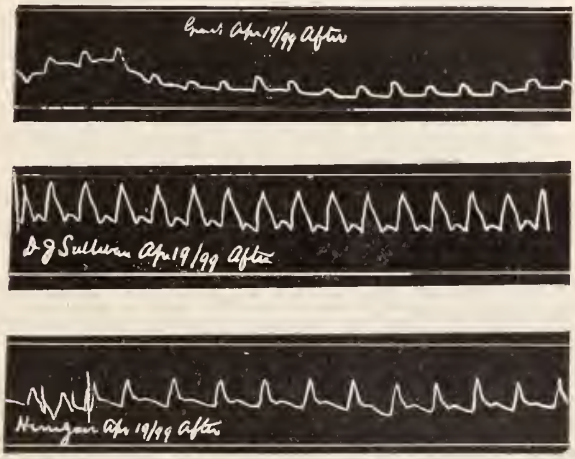


PLATE VIII.—Nos. 2, 7, and 11. After race, showing low arterial tension.

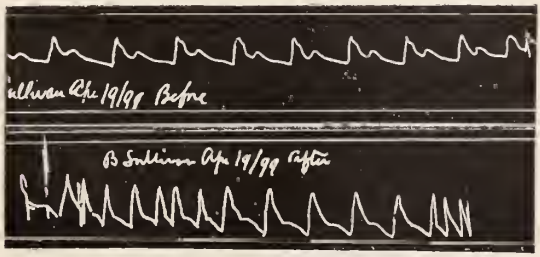


PLATE IX.—No. 3. Before and after.

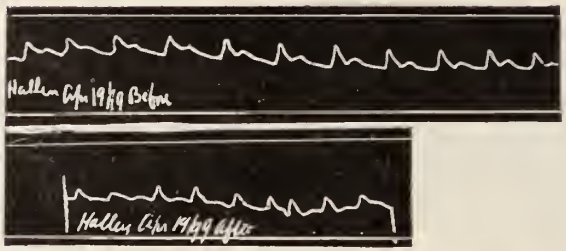


PLATE X.—No. 5. Before and after.

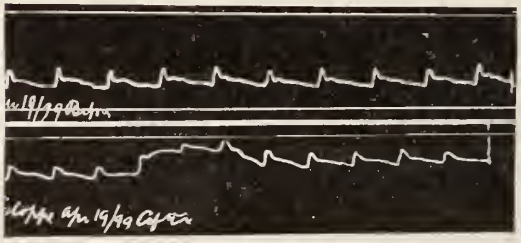


PLATE XI.—No. 6. Before and after.

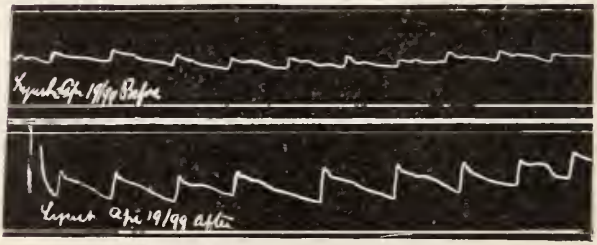


PLATE XII.—No. 8. Before and after.

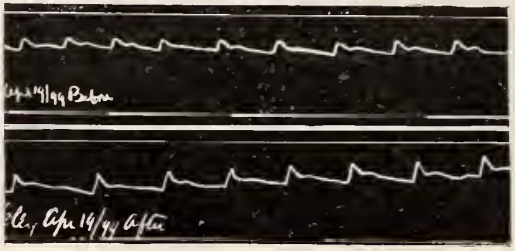


PLATE XIII.—No. 9. Before and after.

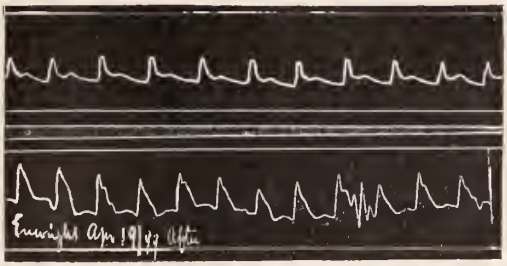


PLATE XIV.—No. 10. Before and after.

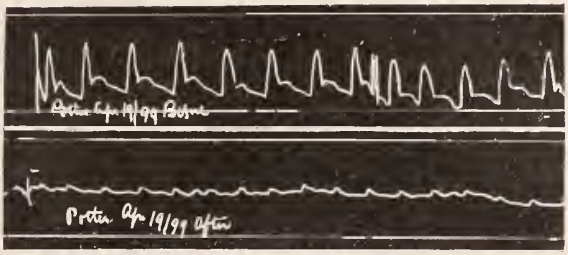


PLATE XV.—No. 12 Before and after.

That the effects as stated do result in individual cases, under certain circumstances and conditions, is undoubtedly true, but the logical fallacy of arguing from the particular to the general is in no way more strongly emphasized than in regard to these conclusions, drawn with respect to the effects of muscular exercise upon the heart.

The conditions existing in the case of a middle-aged sedentary man ascending a mountain and a carefully prepared athlete running a twenty-five mile race are no more similar than are those existing between a new and strong pump subjected to extremes of varying pressure and one which performs its monotonous but temperate function with a worn and leaky valve. In judging the effects of exercise upon the heart, every participating individual and every form of exercise should receive special consideration.

The case of No. 4, Plate VI., compared after a practice-run and after the race, is an excellent illustration of this truth.

After a practice-run of five miles he showed a temperature of 98°, pulse 100, respiration 32, a faint trace of albumin, and a sphygmographic tracing of high arterial tension. After the race he showed a loss of 4.9° in temperature and of $4\frac{1}{2}$ pounds in weight; respiration 24, $\frac{1}{10}$ per cent. of albumin, with numerous casts, and a pulse of 104 and of exceedingly low tension.

One of the most interesting of the conclusions of the examination is the evidence of the preparatory enlargement that the heart undergoes, showing the truth of Osler's statement that "no man becomes a great runner or oarsman who has not naturally a capable if not a large heart." This hypertrophy was shown in these cases by the bulging left intercostal cartilages; the thrusting force of the apex-beat; the enlarged percussion-area; the booming heart-sounds; the high arterial tension, and the increased area of cardiac dulness.

In drawing conclusions from the data given, remembering also that we are solely considering the effects of long-distance running upon the heart of healthy and strong men, and freeing our minds from preconceived notions and theories, the whole result of the examination can be summed up as showing an extreme condition of general muscular exhaustion. In this condition the heart, a muscular structure, participates with the other muscles. Its muscular valves, the mitral because of their most essentially muscular structure, are the first affected, and soonest fail to perform their function, thus permitting regurgitation of the blood into the left auricle. When this regurgitation is extreme, it may even go so far as to lead to backing up of the blood sufficiently to cause stress upon the right heart (*vide* the jugular pulsation which was present in one of the cases, No. 9, Plate XIII.).

In regard to the timing and placing of the murmurs heard in the position named, the anatomic situation of the heart-valves must be borne in mind. Anatomically, the periphery of all these valves can be included in the circumference of the large mouthpiece of the ordinary stethoscope. Anatomically, the semilunar, the mitral, and the pulmonic are included in

a still smaller circle, and the value of faint murmurs heard in the second left intercostal space must be judged by their transmission by the blood-currents and such other favorable sound-conductors as the hypertrophied muscular tissue of the left ventricle and the residual blood in its dilated cavity. The conduction of these murmurs heard in this position in the direction of the hypertrophied left ventricle and their systolic synchrony; the fact that in some instances they were conducted to the apex; that they were heard in the back; together with the increased dulness over the left ventricle, showing slight dilatation, and the sphygmographic corroboration afforded in the five cases, would seem conclusive evidence that the common-sense explanation of what happened in these fourteen tired and exhausted hearts was muscular incompetence of the muscular mitral valve, which was physically unable to perform its function because of its participation in the general muscular weakness. Another significant fact was that those winning first and second place had no murmurs, showing a greater relative muscular vigor of the heart.

In corroboration of this view the researches of Hesse are of value. Hesse has shown that during diastole the mitral orifice is too large to be closed by the mitral valves alone. The orifice must first be constricted by the sphincter-like action of the circular muscular fibres around it. Probably with the muscular incompetence of this sphincter there is also associated a muscular incompetence of the left papillary muscles, which muscles are much larger and stronger than those of the right side. Also, it is probable that the muscular fibres of the left ventricular wall, which is thicker and stronger than the right, bear their proportionate share in this relative muscular weakness. Indeed, it would seem that nature, which had constructed this valve after a distinctly more muscular pattern, had provided a physiological safety-valve for long-continued muscular strain. It is not our intention to enter into the discussion of Balfour and Russell upon the significance of murmurs heard in the so-called pulmonic area. Suffice it to say that while we agree with Balfour in his

deductions as to the significance of these murmurs, we must agree with Ball, of Philadelphia, in our belief that the anatomic basis of Balfour's deductions founded upon Naunyn's observations is "not proven." In other words, while we regard these murmurs as mitral we do not feel absolutely convinced that they are transmitted by the left auricular appendage. In these cases we believe them to be transmitted by the hypertrophied ventricular wall.

The dictum of Sansom, that a systolic murmur heard at the apex and in the back fulfils all the requirements of a mitral organic murmur, is a statement upon which we are ready to rest our case. Six out of eleven fulfil all the requirements of authorities as to mitral murmurs. Are we not justified by the premises and by the exactly similar conditions in the conclusion that the five other murmurs are also mitral, differing only in degree?

To sum up the significance of those heart-murmurs, it seems to me conclusive that they are simply to be regarded as the participation of the muscular mitral valves in the general muscular exhaustion.

With respect to the effect upon the individual of such contests, a question we are often asked, we can only say that we do not see how such a contest can be in any respect more injurious than any other form of exhausting muscular exercise, such as long-distance bicycle-riding, swimming, and the like. The injurious effects, if any, are due to the supreme stress of the contest and not to the nature of the exercise. Long-distance running under suitable conditions and without the contest should be beneficial rather than otherwise. Personally I regard it with the contest as far less injurious than other practices indulged in by exuberant young men.

Bibliography.

American Text-book of Physiology.
Osler. Practice of Medicine, 1898.
Broadbent. Heart Disease, 1897.
Prince. New York Medical Record, April 20, 1889.
McCollom. Boston Medical and Surgical Journal, 1889.
Balfour. Clinical Lectures on Diseases of the Heart and Aorta, 1898.

Sansom. The Diagnosis of Diseases of the Heart and Thoracic Aorta, 1892.
Gibson. Diseases of the Heart and Aorta, 1898.
A. Brayton Ball. New York Medical Record, April 12, 1884.
Glentworth R. Butler. TRANSACTIONS AMERICAN CLIMATOLOGICAL ASSOCIATION, vol. iv., 1898.
Allbutt. St. George's Hospital Reports.

DISCUSSION.

DR. THOMAS DARLINGTON: I hope that Dr. Williams' paper will appear complete in the TRANSACTIONS of the Association, together with the drawings he has made. The paper is certainly a very valuable one, and worthy of careful study.

DR. NEWTON: I saw a boy who had acute dilatation of the heart from throwing a hammer. He recovered entirely, so far as I could learn. I would like to ask Dr. Williams if he has made any observations upon the subsequent condition of these men. I would also like to ask how long their depression lasted, as well as the temperature and heart-murmur.

DR. WILLIAMS: The murmur lasted only a very short time—from two to three minutes to half an hour. One of the contestants rode fifteen miles on his wheel after the race, while a second one took part in a one-mile race eight days later, and, although on that occasion he accidentally fell in rounding the track, he finished within two seconds of the world's record, which shows that he could not have been much injured by his previous race in Boston. Several of the men had been participants in previous races. The blood-count was so unsatisfactory that it was discontinued. The men when they came in were bathed in sweat, and they varied much in the degree of anæmia and hyperæmia of the skin.

THE RELATION OF LOCAL METEOROLOGICAL CONDITIONS TO THE INFLUENZA EPIDEMIC IN PHILADELPHIA, WINTER OF 1898–99.

By HOWARD S. ANDERS, A.M., M.D.,
PHILADELPHIA.

The rapidity and extensiveness of the spread of influenza have been its conspicuous characteristics ever since its recognition as a peculiar, if not a specifically definite, epidemic disease.

That the very name *influenza*, introduced by the Italian writers probably as early as the fourteenth century,* and retained and used even now, is appropriate seems quite clear, and at once indicates more or less belief in the surrounding atmosphere as being either actively or passively productive of the affection. The French term *la grippe* and English (or United States) *grip* are equally suggestive, however, in the clinical sense.

The problem involving a study of the meteorological conditions relative to the etiology of epidemics of influenza is essentially a large and intricate one, for it means the investigation and analysis of accurately recorded data for urban and rural localities in connection with a given epidemic; a comparative study of both antecedent and associated meteorological conditions observed in a series of localities during epidemic and non-epidemic years—*i. e.*, to learn whether these conditions in one place are similar to those of other places; a comparison

* By English writers in the eighteenth century.

of the given observations of a given locality during one epidemic with those of the same place for corresponding periods of the year visited by other epidemics, and so on.

Moreover, it would be desirable and helpful to know what, if any, special weather states bear a causative relation to influenza, either in predisposing individuals to the disease, or in encouraging the cultivation of Pfeiffer's bacillus ready for active invasion, or both. In short, the problem is primarily and, for the present, purely scientific. Is it practical also?

Finkler[1] says: "We cannot, however, *at the present time* (italics mine) at least, predict the appearance of an influenza epidemic from any existing atmospheric conditions." But is it not really likely that a careful study may afford the induction of principles that may have at least hygienic value, just as storm signals, when heeded, may save lives and property?

From the title of this paper it will be obvious that the results here put forth are but a beginning—a preliminary paper which, God willing, it is hoped to have followed by further work along the lines stated.

We are met at the outset by such a variety of dicta concerning the idea of atmospheric conditions influencing the production or aggravation of "grip" that, on *à priori* grounds, there are certainly no sound or sure premises upon which to argue a safe and stable conclusion. We must endeavor and continue to maintain a mental attitude of wholesome skepticism throughout the laborious attempt at solving a problem that may have an answer either positive or negative.

Schoenbein attributed influenza to the ozone in the atmosphere.

Flint[2] wrote: "The special cause doubtless exists in the atmosphere, and is independent of appreciable atmospheric changes. This fact was exemplified in the Massachusetts General Hospital during the extensive prevalence of this epidemic in 1832. Nearly all the patients in the hospital were affected, although the wards were kept day and night at a uniform temperature."

The late Dr. Pepper[3] made the following sweeping nega-

tion: "There are no known conditions of climate, soil, elevation, or season which affect it." Osler[4] has it thus: "It appears to be independent of meteorological conditions." J. M. Anders says:[5] "The affection is only slightly influenced, if at all, by meteorological conditions."

Seminola (*New York Medical Journal*, April 11, 1891), while assuming the parasitic nature and origin of influenza, nevertheless suggests: "But for all that there may be cosmotelluric conditions, undetermined and undeterminable, capable of producing very severe disturbances."

Dr. Goodhart[6] believes that the supposed atmospheric origin of influenza is unworthy of belief, but that it is contagious by man and fomites and spares no climate. Yet he doubts not that some atmospheric influences must exist that "make for an epidemic;" and suggests that certain conditions of environment may obtain by which the reproduction of lower orders of life has been extraordinarily facilitated, and that meteorological influences operate is quite probable from the common observation of the effect of low barometer on corns. "And as in all zymotic diseases, even endemic ones, there are times when the disease is violently active—times when it sleeps and seems about to disappear, the condition of its life-history remaining constant all the time—there is no need to labor the proof of epidemicity. We know nothing whatever, and are a long way off the discovery of subtle influences of this kind, but none the less they certainly exist."

In this connection it will do no harm to quote the radical assertion of G. V. Poore recently in one of the Milroy Lectures before the Royal College of Physicians, London: "'The proper study of mankind' is not man but what goes on in nature outside his body, for here is where we must look to get to the bottom of the problem of infectious disease, especially when it takes an epidemic form."

Coming to a more definite opinion, we find Curtin and Watson,[7] in a conjoint study of the epidemic of 1893–94, expressing their belief that fogs and a warm, humid atmosphere seemed to favor the spread of influenza in ancient epi-

demics, and that there is a form of disease always present, "as the cold we 'catch' under various atmospheric causes, which under vicious atmospheric conditions and climatic variations is capable of developing in one or another direction" —*i. e.*, of becoming epidemic.

As an unwarrantably extreme view, indefinite and unsupported by facts, mention may be made of Kline's[8] allegation concerning "a sudden and extreme change in the barometric pressure—a change in temperature and an increased development of electricity—and, as a result, a greatly increased quantity of ozone" (as Schoenbein). He thinks that "the remote causes are the physical conditions of the earth and air, the more immediate are the results of these forces."

The epidemiological history of influenza shows that the fall and winter months were those during which its sharp outbursts and extensive prevalence were most marked, as far back as the twelfth century even, particularly in England. Thus Thomas Short, writing of the epidemic of 1557, says: "It was a very unseasonable year in England. . . . In the end of September came a very strong, cold north wind, presently after there were many catarrhs, quickly followed by a most severe cough, pain of the side, difficulty of breathing, and a fever." Sydenham reports the epidemic of 1675 to have followed a sudden frost in October, which was preceded by a long period of clear weather. Webster speaks of Philadelphia being attacked in the winter of 1760–61.

Hufeland, writing of the 1833 influenza epidemic, believed it to be due to air infection. Hagen's[7] opinion (epidemic of 1857–58) was that influenza was miasmatic contagious, the contagion developing in the patient and accumulating in the air, but not being directly transmissible to another person.

Finkler, of Bonn (*loc. cit.*), says: "We know that the pandemic is not more quickly disseminated than would be possible by our most rapid means of travel, the railroads and the ocean steamers." Nevertheless, he acknowledges that owing to the character of the disease and its selection for the respiratory organs, the weather is an important factor, and

can only account for the exceptional instances of rapid spread. It should be recalled just here that the bacillus of Pfeiffer is strictly aërobic.

One's intellectual or intuitive temptation to infer the atmosphere to be either actively or passively concerned in the diffusion of influenza is, to a degree, rational when one considers how the malady attacks suddenly, over large areas of city and country, "all sorts and conditions of men," especially those exposed out of doors—the strong man and the weak, the young and old, the comfortable and unfortunate.

While some authors admit, then, that certain atmospheric states or movements may exert an influence on the dissemination, if not the production, of this disease, what special meteorological conditions thus favor it we do not know. So far as I know, only within the past six months has any particular study of the subject been reported. I refer to the work of P. Dignat,[9] in Paris, last November. While my own data were being gleaned my attention was directed to his analysis of the concomitant meteorological conditions to influenza since 1895. He considers an abnormal increase in the barometric pressure, abnormal temperature ranges, lowering of the electrical influence, abnormal predominance of north winds, and weakening of the actinometric degree to be the meteorological conditions that precede epidemics of influenza.

However, it should be pointed out that in connection with the great pandemic of November and December, 1889, Assmann observed a very unusual drought in Eastern and Central Europe, the absence of a protecting covering of snow, and low-lying clouds and high barometer.

Since the fourteenth century there has been a sort of grouping of periods of from two to eight or ten consecutive years when influenza was more or less markedly epidemic, these periods being separated by longer intervals, just as we have had several epidemics since the great visitation of 1889–90. So that while it is nearly certain that, like poverty, tuberculosis, taxes, and sin, influenza is always with us, it has its stormy waves of epidemicity and its overwhelming

tidal waves of pandemicity. The difficulty of comparing the weather observations of years of marked prevalence with those of absence of the disease is thus manifest. Hence, in this local and limited research the tabulated data of the recent epidemic in Philadelphia are compared with those of the three previous years of milder prevalence. In a future communication I hope to bring the results of a more extended analysis and comparative study of these observations as they may be related to the great pandemic of 1889–90 and to still earlier sporadic years.

The figures given were transcribed from the observation records made and kept in the Philadelphia station of the United States Weather Bureau, and I take sincere pleasure in acknowledging my gratitude to Mr. Luther M. Dey, local forecast official, and to his assistants, Messrs. Doherty and De Graw, without whose kind permission, constant courtesies, and ready explanations the most of this work would not have been as possible, profitable, and pleasurable as I found it to be.

TABLE I.—ATMOSPHERIC PRESSURE (INCHES).

Year.	Barometer.	October.	November.	December.	January.
1899	Mean monthly	.....	.....	.....	30.134
	Highest	.....	.....	.....	30.907
	Lowest	.....	.....	.....	29.329
	Range	.....	.....	.....	1.578
1898	Mean monthly	30.096	30.051	30.034	29.918
	Highest	30.415	30.450	30.446	30.426
	Lowest	29.428	29.252	29.063	29.253
	Range	0.987	1.198	1.383	1.173
1897	Mean monthly	30.030	29.994	29.979	30.021
	Highest	30.506	30.490	30.44	30.584
	Lowest	29.543	29.318	29.17	29.459
	Range	0.963	1.172	1.26	1.125
1896	Mean monthly	29.940	30.102	30.074	30.074
	Highest	30.383	30.645	30.79	30.447
	Lowest	29.518	29.482	29.51	29.572
	Range	0.865	1.163	1.27	0.875
1895	Mean monthly	29.942	30.067	30.028	
	Highest	30.462	30.548	30.64	
	Lowest	29.429	29.325	29.44	
	Range	1.033	1.223	1.20	

Referring to the table above, it will be seen that in the recent epidemic of influenza (1898–99) the mean monthly

atmospheric pressure was moderately high for the two months (October and November) preceding the marked outbreak, although quite a number of sporadic cases, especialy in the latter month, were noted. During the month of rapid development (December) the pressure mean was also moderately high. The highest mean occurred in January, while the disease was most severe and generally prevalent (except the last week); the extremely high pressure of 30.907 having been reached on the second day of the month.

If the calculations mentioned by Prof. Davis,* of Harvard, be correct, namely, that the mean pressure over the whole earth for the year is 29.89 inches, and for the northern hemisphere for January 29.99 inches, and when it is realized that increments or decrements of a few tenths or hundredths of an inch may involve changes of potential significance in the atmospheric mass, the barometric figures given above deserve careful consideration.

Comparing the mean monthly pressure of these epidemic months of 1898–99 with that of the corresponding months of 1897–98, when influenza was very lightly prevalent, the diminished pressure for the latter is obvious; although the highest barometric readings for the corresponding months were about the same, except for January.

In 1895–96 and 1896–97 the mean monthly atmospheric pressures for November, December, and January were also moderate, and yet the "grip" was not intense, though present to a degree. In January, 1897, this affection manifested a brief exacerbation, and we note that the barometer registered the high point of 30.584 inches (on the 31st), preceded by the November and December, 1896, highest points of 30.645 and 30.79 inches, respectively.

Perhaps the most marked feature is the distinctly greater absolute range between the highest and lowest atmospheric pressures for the epidemic months (December and January) of 1898–99, as compared with those of 1895–98, showing a probable relation of prevalence to extreme pressure changes,

* Elementary Meteorology, p. 92.

the lowest readings being (with one exception—viz., January, 1899) lower than the lowest for the corresponding months of the latter years.

TABLE II.—MEAN MONTHLY TEMPERATURE.

Months.	1899	1898	1897	1896	1895
October	.....	58.6°	56.1°	52.4°	51.2°
November	.....	44.6	44.8	49.2	45.4
December	.....	35.9	36.6	33.9	37.8
January	32.3°	34.5	29.8	30.2	

The mean monthly temperatures show that it was slightly colder during the epidemic months (December and January) of 1898–99 than during the corresponding months of the preceding winter, but not quite so cold as the winter of 1896–97, when influenza was again somewhat epidemic, and, on the average, about equal in temperature with the same months of 1895–96. It is to be noted that the mean monthly temperature for October preceding the recent epidemic was rather high comparatively, as was also that of November, 1896, in both instances about two months before the height of the influenza epidemic.

TABLE III.—TEMPERATURE—MEAN MONTHLY MAXIMA AND MINIMA.

Months.	1899		1898		1897		1896		1895	
	Max.	Min.	Max.	Min.	Max.	Min.	Max.	Min.	Max.	Min.
October . . .	.....	.....	66.5°	50.7°	66.9°	49.6°	60.2°	47.0°	61.3°	44.0°
November . .	.....	.....	51.3	37.8	53.1	38.7	58.3	42.5	53.8	39.8
December . .	.....	.....	42.0	29.8	44.4	31.8	41.3	28.2	46.0	32.3
January . . .	39.5°	25.1°	41.2	29.5	37.3	24.2	36.6	25.5		

This table (No. III.) gives the means of or average differences between the highest and lowest temperatures for the months noted, to be discriminated, therefore, from the absolute and greatest daily ranges of temperature for each month.

During the greatest activity of the recent "grip" epidemic (December, 1898, January, 1899) both the maxima and minima of temperature were lower than for the same months of the previous winter, when "grip" was much less prevalent; but not so low, quite, as in 1896–97, when influenza was also markedly in evidence.

An exceptionally low mean maximum for a mild influenza year is observed for January, 1896, although this is counterbalanced somewhat by the relatively much higher means, both maximum and minimum, for December, 1895.

Of the preceding autumn months of October and November nothing characteristic seems to mark the data unless it be the rather close correspondence between the figures for the mean maxima and minima of October for the years 1897 and 1898, respectively.

Regarding temperature extremes for the month of December, the *absolute ranges* (between the highest and lowest points recorded) for the years 1898, 1897, 1896, and 1895 were 44°, 47°, 48°, and 53°, respectively; while the *greatest daily ranges* for the same month of these years were 35°, 25°, 21°, and 28°, respectively.

For January, 1899, and the three preceding years the *absolute ranges* were 50°, 46°, 54°, and 49°; the *greatest daily ranges* were 29°, 27°, 25°, and 20°.

From these figures it will be seen that the absolute ranges of temperature bear no characteristic relation to decidedly epidemic years, on the one hand, or to merely sporadic or mildly epidemic years, on the other, unless we except January, which shows a greater absolute range in the former than in the latter. But any relative importance that absolute ranges of temperature throughout given months and years may have in such a study as this is at best only problematical and indefinite.

Furthermore, while the greatest daily ranges for the two months (December, 1898, and January, 1899) of the height of the influenza epidemic in Philadelphia show marked extremes between the maxima and minima as compared with

preceding years, their direct relationship will depend most probably upon the dates of their occurrence—their suddenness, rapidity, and frequency of movement—data not obtainable for this article, with one exception. The latter shows that both greatest daily ranges, of 35° and 29°, occurred on December 31, 1898, and January 10, 1899, respectively, while la grippe was most violent.

However, the table above and that following, giving the means of more extensive observations, are evidently better criteria of temperature conditions.

Finally, I found that taking the actual number of days that the mean temperatures were above 50°, 59°, and 68° (U. S. Weather Bureau Monthly Records, Philadelphia Station), October, 1898, was an unusually warm month compared with the four years immediately preceding; November, however, was decidedly colder in 1898—two days of mean temperature below 32°, and seven days of minimum temperature, also below 32°, having been recorded; December showed great comparative coldness, too, not only in the small number of days of mean temperature above 41° and 50° (three and one, respectively), as contrasted with previous years, but in the fact that there was one day in which the maximum temperature was below 32° and seventeen with a minimum below 32°; in the same way, January exhibited a distinctly, though less decidedly, relative increase of coldness.

TABLE IV.—TEMPERATURE—MEAN DAILY RANGE AND MEAN VARIABILITY.

Months	1899		1898		1897		1896		1895	
	Mean daily range.	Mean variability.	Mean daily range.	Mean variability.	Mean daily range.	Mean variability.	Mean daily range.	Mean variability.	Mean daily range.	Mean variability.
October . . .	.....	.....	15.8°	3.6°	17.2°	5.1°	13.2°	3.6°	17.3°	4.3°
November . .	.....	.....	13.5	3.3	14.4	6.0	15.9	6.8	14.0	6.4
December . .	.....	.....	12.2	4.2	12.5	4.9	13.1	4.6	13.7	5.5
January . .	14.4°	6.9°	11.6	4.7	13.1	6.2	10.9	4.4		

The *mean daily range* and *mean variability* afford a very fair basis for estimating the equability of temperature in various localities and at different times. The mean variability represents the monthly mean of the day-after-day differences in maxima and minima or daily ranges of the dry bulb thermometer.

Analysis of the table shows, contrary to what one might expect, that the months of November and December, 1898, were more equable than for the other years; but that January, 1899, was decidedly non-equable.

TABLE V.—RELATIVE HUMIDITY—MEAN MONTHLY (PER CENT.).

Months.	1899	1898	1897	1896	1895
October	.....	75.4	72	73	61
November	.....	74.2	72	75	78
December	.....	70.0	77	68	70
January	71.2	72.0	74	71	

It is usually stated that influenza prevails, by selection almost, in cold, damp weather—rather vague terms, and totally lacking in scientific precision or rational discrimination. The history of influenza epidemics shows that the disease may spread in warm or cold, dry or damp weather, yet the rapidity, intensity, and universality of spread may be dependent in a measure upon certain sequences and associations of atmospheric temperature and moisture.

It is certainly true that the temperature of the air that we *feel* is very much influenced by the amount of vapor it contains at a given time—*absolute humidity*. But it is much more appropriate and important to determine the *relative humidity*, which expresses the percentage of saturation or the proportion of aqueous vapor actually present, compared with the amount of vapor that might exist if the air was saturated at the temperature taken.

A glance at the table above shows the average mean relative humidity for October (exactly the same as August, 1898)

and November, 1898, to have been distinctly higher than for 1897, and a little higher than in 1896, when influenza was mildly epidemic, and again markedly higher than in 1895 (also an "off" year like 1897); the mild influenza year of 1896 also shows a higher relative humidity for these breeding(?) months, as compared with the sporadic years 1895 and 1897.

On the other hand, while "grip" was at its height during December and January of the recent prevalence, the mean percentages of saturation were lower than for the two preceding winters and practically the same as for 1895–96.

Just here it should be stated that the *mean monthly dew-points* have been directly in accord with the variations of relative humidity—*i. e.*, comparatively high in October and November and low in December and January for the years tabulated.

As a test of the mean monthly relative humidity table the hygrometric records (means of the daily 8 A.M. and 8 P.M. observations) for the month of December only were classified as follows:

TABLE VI.—RELATIVE HUMIDITY BY DAYS.

Date.	Number of days percentage above			At 100
	70	80	90	
December, 1898	15	9	6	2
" 1897	23	12	6	3
" 1896	14	4	1	0
" 1895	16	9	4	0
Means . . .	17	8 5	4.25	1.25

This corroborates the statement regarding the lower relative humidity for the time of greatest development during the influenza epidemic of the past winter, as well as of the beginning development of that of 1896–97, compared with the alternate years of slight disturbance.

TABLE VII.—PRECIPITATION (INCHES).
Total amounts.

Months.	1899	1898	1897	1896	1895
October	.....	4.85	1.70	2.08	2.97
November	.....	7.19	4.44	2.50	2.32
December	.....	3.21	4.52	1.00	1.76
January	4.01	4.10	2.17	1.57	

Any possible etiologic relation that the amount of precipitation might have upon the epidemicity of influenza would seem to be connected with the soil rather than the air; and assuming such a telluric relationship to be true, reasoning *à priori*, we would infer that the influence of a dry or wet soil, as the case might be, would be a predisposing rather than an exciting one —a good culture medium for the bacilli of Pfeiffer, as it were.

The question arises, then, whether the considerable rainfall of October and November, 1898, had any effect in preparing a rich and robust crop of influenza microbes. A relatively moderate amount of precipitation was also noted in the fall of 1896 and 1895.

It may be pertinent to this matter also to mention the fact that last September (1898) was dry, the total rainfall having been but 1.82 inch; while August, on the contrary, was remarkable in that the total precipitation was 9.06 inches, 5.43 inches of which fell within one hour and forty-four minutes.

TABLE VIII.—WIND—PREVAILING DIRECTION, TOTAL MOVEMENT, AND VELOCITY (MILES).

Month.	1899			1898			1897			1896			1895		
	Prevailing direction.	Total movement.	Av. hourly velocity.	Prevailing direction.	Total movement.	Av. hourly velocity.	Prevailing direction.	Total movement.	Av. hourly velocity.	Prevailing direction.	Total movement.	Av. hourly velocity.	Prevailing direction.	Total movement.	Av. hourly velocity.
Oct.	.....	...	...	N.W.	7250	9.0	N.E.	7978	10.7	N.	7618	10.2	N.W.	7999	10.8
Nov.	.....	...	...	N.W.	7354	10.2	N.W.	6998	9.7	S.W.	7097	9.9	N.W.	7195	10.0
Dec.	.....	...	...	S.W.	7857	10.6	N.W.	7563	10.2	N.W.	7281	9.8	N.E.	8492	11.4
Jan.	S.W.	7942	10.6	N.W.	7837	10.5	N.W.	8422	11.3	N.W.	8121	10.9			

The *prevailing direction* of the wind (from the west) was generally opposite to the direction (westward) of travel of the epidemic. Most of the time the wind blew from the northwest during the months tabulated, except those of the greatest prevalence of influenza last winter, when it was southwest. It should be noted, however, that in December, 1898, the wind blew from the northwest almost as much as from the southwest. From the northeast and east—that is, more in the trend of the spread of the disease—the wind was observed to blow from 14 to 24 per cent. of the time throughout the four months (see table below).

TABLE IX.—WIND—DIRECTION AND PERCENTAGES, 1898-99.

Months.	Percentage of time wind observed blowing from									
	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Easterly.	Westerly.
October	13	13	11	16	10	8	10	19	40	37
November	8	10	7	5	13	12	10	35	22	57
December	6	11	3	3	7	28	16	26	17	70
January	21	10	6	5	10	23	19	6	21	48
Totals	48	44	27	29	40	71	55	86	100	212

Whence it is apparent at once that the westerly winds prevailed more than twice as often as the easterly, especially during the heavy "grip" month of December, more than four times as often. Certainly influenza, unlike a United States weather map arrow, does *not* "fly *with* the wind," in its *prevailing direction* at least.

However, since westerly winds are usually associated with high barometer, low temperature, and low relative humidity, it is not unreasonable to infer that in some way, by their combined effect upon either the human anatomic or the telluric soil, the disease and its germs are favored in their propagation and multiplication.

The *total movement of wind* in miles (Table VIII.) was

relatively greater during the recent epidemic than for the same period of 1897–98; and also for January, 1897 ("grip" prevailing), as compared with January, 1896. But the preceding month of October shows correspondingly and considerably less total wind movement.

The *average hourly wind velocity* was only slightly higher for the epidemic months of last winter as compared with the same months of the year previous. It is more likely that the degree of *maximum velocity* of the wind at certain times may bear some relation to the spread of influenza. Thus, to begin with, the weather records show that October, 1898, was unusually calm and free from high winds as compared with former years, the maximum hourly velocity for the month having been but twenty-seven miles, which was decidedly lower than for at least nine years previous, and this on the sixth day of the month, some time before "grip" became epidemic.

On the other hand, not only was November, 1898, characterized by greater total wind movement than for four years prior, but by increased high winds—principally northwest—above twenty-five miles an hour, on the 8th, 11th, 20th, and 27th days of the month (26, 36, 28, and 42 miles per hour respectively), while sporadic cases of influenza were beginning to multiply rapidly. The maximum hourly velocity for the month—42 miles on the 27th—was higher than any that had been reached for seven years.

Influenza became virtually an epidemic in the first week of December, 1898, and after the high wind, noted just above, came the maximum wind velocity of this month, on the fourth day, from the southeast, at the rate of 48 miles per hour—higher, with one exception (1895) than any year since 1890. The next day the wind blew from the west at 25 miles per hour. Again, during the height of the epidemic, the dates and rates were as follows: 27th, 27 miles; 28th, 30 miles; 31st, 26 miles; the wind coming from the northwest on the first two days and from the north on the last day.

TABLE X.—GENERAL WEATHER CONDITIONS.

Number of days.

Months.	1899				1898				1897				1896				1895			
	Clear.	Partly cloudy.	Cloudy.	Foggy.	Clear.	Partly cloudy.	Cloudy.	Foggy.	Clear.	Partly cloudy.	Cloudy.	Foggy.	Clear.	Partly cloudy.	Cloudy.	Foggy.	Clear.	Partly cloudy.	Cloudy.	Foggy.
Oct.	...	...	...	...	11	8	12	2	13	4	14	2	9	7	15	2	21	5	5	0
Nov.	...	...	...	...	8	11	11	2	5	14	11	3	9	10	11	2	11	6	13	4
Dec.	...	...	...	...	10	8	13	3	5	13	13	2	11	12	8	0	10	7	14	0
Jan.	10	8	13	2	5	13	13	2	10	7	14	1	10	10	11	0				
Totals for the seasons					39	35	49	9	28	44	51	9	39	36	48	5	52	28	43	4

It is interesting to note that there were twice as many clear days during the epidemic months of December and January as there were for the same months of 1897–1898, and fewer partly cloudy days, but the same number of cloudy days and one more foggy day. November, 1898, was also more sunshiny than November, 1897.*

And yet, when one considers that less than one-third of the days were clear, and that the hours of sunshine are very few during the short days of the winter solstice, it seems not improbable that somehow, on the whole, "la grippe" likes the darkness better than the light, because certain conditions for its ravages are more favorable then than at any other time of the year. Otherwise, the relatively greater number of clear days during the recent epidemic is perfectly consistent with the other meteorological phenomena that seem to be associated with the spread of influenza—namely, that cold, dry, windy (westerly) weather and sunshine go together.

Finally, attention may be called to the fact that the number of clear, partly cloudy, and cloudy days of December, 1898, and January, 1899, were exactly equal. My personal observations during the development and height of the late epi-

* The rare experience of a midwinter thunder-storm was had on Dec. 22, 1898.

demic, without any endeavor to obtain precise meteorological data were generally as follows: (1) That during November and December, 1898, there were several volleys of attack, as it were, in which influenza suddenly overcame numbers of people, for three or four days or a week, perhaps, and then seemed to subside to a degree, to be followed by other exacerbations and remissions of invasion; (2) these attacking periods were invariably preceded by sudden thaws, "thick, ill-smelling" fogs, and relatively warm, moist, and calm weather; (3) they were apparently introduced by and associated with cold, clear, dry, and windy weather; (4) these changes appeared to be unusually sudden, extreme, frequent, and rapid. I was impressed particularly by the character of the early evening fogs, with their intensely carboniferous odor and suffocative heaviness. Physicists, accepting the theory that each tiny mist particle has a portion of soot or smoke or dust for its nucleus, would seem to have a rational explanation to account for fogs, and the phrase "climate of cities" is truly fitting. It is a fair question to ask whether the sporadic cases of influenza in non-epidemic years occur only in large towns or cities and thickly populated districts, and whether their local peculiarities of weather are directly or indirectly concerned in their production.

As hinted at the beginning of this paper, we have to face these queries: Do weather conditions in general prepare for or sustain epidemics or pandemics? Are there special atmospheric phenomena that increase the susceptibility and weaken any possible immunity of our bodies to influenza, or that facilitate the growth and multiplication of the bacilli of Pfeiffer, or both? Or, is it more probable that meteorological occurrences influence the former more and telluric states the latter more?

Future studies of future epidemics and, comparatively, of normal years, over widely scattered places of the "grip" portions of the globe, by careful observers only, will enable us to know precisely the answers to these queries.

Any summary that the data of this unfinished study may

justify is practically in accord with my statements 2 and 3, given above.

The conclusion of the whole matter thus far is interrogatory and problematical: Will future and further investigations along these lines serve to confirm, add to, modify, or refute these preliminary results? *Das Kommt darauf an!*

Bibliography.

1. Finkler. Twentieth Century Practice of Medicine, vol. xv.
2. Flint. Practice.
3. Pepper. American Text-book: Theory and Practice of Medicine.
4. Osler. Practice.
5. Anders, J. M. Practice.
6. Goodhart. Article on Influenza in Allbutt's System of Medicine.
7. Curtin and Watson. Trans. Philadelphia Co. Med. Soc., 1894, vol. xv.
8. Kline. Trans. Med. Soc. State of Pa., 1895, vol. 26.
9. Dignat. Journ. de Méd. de Paris, 18me Année, Nov. 9, 1898, vol. x. No. 45. Abstract in Phila. Med. Journ.

THE COLD WAVE OF FEBRUARY, 1899.

By GUY HINSDALE, M.D.,
PHILADELPHIA

While great interest attaches to the areas of low barometric pressure as they pursue their course across our continent, the areas of high pressure are not without their interest and importance. In the area of low pressure resides the cyclone, while the area of high pressure determines the anticyclone. A cyclone is an entirely different thing from a tornado. It is a very broad storm, oftentimes a thousand miles in diameter, and sometimes can be followed half around the world. The winds of the cyclone are at a distance from its centre, and occasionally rise to the violence of a hurricane; but they are not to be compared with the extreme violence of the tornado, as, for instance, the recent one in Kirksville, Missouri, before which the most solid structures were razed. The cyclone is accompanied by clouds and rain; the anticyclone is cloudless, bright, and invigorating. However much more to be desired the anticyclone may be than its opposite, there are yet some things to be feared from even its advent. I refer to the great cold waves that in winter sweep across the continent.

We all recall that after some especially severe storm in winter the skies clear, the wind dies away, and on the following morning the still, sharp air, azure sky, and the creaking of wheels proclaim that the mercury is near the zero mark. That is the winter anticyclone. Stillness instead of the whirlwind, dryness instead of moisture, intense cold instead of a temperature of 30° or 40° F. These cold

waves are most intense when an area of low pressure has passed to the northeastern corner of the United States, and when, perhaps, another low area is developing in the southwest, the high area being situated in the northwest. If, now, isothermal lines are crowded together, owing to the close proximity of warm air to the colder air of the northwest, we expect a sudden and severe fall of temperature over a large extent of country. To constitute a cold wave, so called, there must be a fall of twenty degrees or more in twenty-four hours, free of diurnal range, and extending over an area of at least 50,000 square miles of country, the temperature somewhere in the area going at least as low as 36° F.

By drawing lines on a map through the places where an equal fall of temperature has occurred certain areas will be enclosed, and these are occasionally of very large extent. In one of the greatest cold waves of recent years, that of February 17, 1883, the temperature at 7 A.M. was twenty degrees lower than at the same hour on the day preceding, throughout an extent over one million square miles, extending from Lake Superior and Georgian Bay, on the north, to the Rio Grande, on the south, and from Kansas City to Cincinnati. Inside the area of twenty degrees fall there was an area of thirty degrees fall of 640,000 square miles; inside of this an area of forty degrees fall of 187,000 square miles; inside the forty degrees fall was an area of 31,000 square miles of fifty degrees fall, and inside of this a sixty degree fall at Keokuk, Iowa, the centre of the cold wave, the temperature of that place being 60° F. on the morning of February 16th, and zero the next day.

In the winter these cold waves are frequent, and, unless exceptional, do comparatively little harm. We expect them to occur, and feel the benefit usually of the invigorating air. But as spring opens and vegetation becomes active, as people are inclined to throw off their clothing of winter, there is a sudden arrest of the warm weather; the sun's warmth, which seemed to favor the growth of crops, which inspired hope in the breasts of thousands of invalids, meets with a rebuff.

The season is reversed, the sun's rays are weak, and everyone is anxious to know the effect of the cold snap on their various interests.

It seems fitting that in an association of this kind we should make a note, at least, of the remarkable period of cold which prevailed over the greater portion of the United States during the second week in February of this year. It was an unparalleled cold wave, and established a new record for the majority of stations east of the Rocky Mountains. Three cold waves covered the period from January 26th to February 14th, and followed extensive snow storms, which caused great distress and loss of life in the Western States. A detailed study of the atmospheric conditions has been made by Mr. A. J. Henry, of the United States Weather Bureau, and published in the monthly *Weather Review* for April. It may be said, in brief, that the snow falls extended as far south as Northern Texas late in January. The Rocky Mountains seemed to act as an effective barrier to the movement of cold air westward, so that after February 7th the condition to be mentioned seemed to have only a moderate influence on the Pacific slope. On February 1st an area of low pressure moved inland from the Pacific, striking the continent about latitude 45°, and proceeded across the great basin toward Southern New Mexico and Southwestern Texas. This low area passing into the interior made it possible, therefore, for the rapid descent of cold air from the areas of high pressure. Just as water flows down hill, so cold air flows from high to low, and the more rapid the sharper the gradient. This was then the keynote of all the changes that followed. This low pressure was accompanied by heavy snow storms.

The second high pressure area started from Assiniboia on February 7th, and uniting with the first mentioned high area remained stationary for a time over Montana until February 11th, when a maximum of 31.42 inches was recorded. It was an offshoot of this high area that moved southeastward over the Upper Mississippi valley, the Ohio valley, the lower

lakes, and the middle Atlantic States, reaching the Atlantic coast, in the vicinity of Chesapeake Bay, on the morning of the 11th. This was a period of intense cold. No record had ever been made of such low temperatures as then prevailed.

On the 11th and 12th the high area, which was stationary over Assiniboia for three days, began moving south, reached Texas on the 12th, and reached Florida on the 13th, and thence took a northeasterly direction up the Atlantic coast, but this latter movement was not accompanied by quite such low temperatures as prevailed on the 11th.

These effects of cold were more pronounced in the Northwest, Central, and Southeastern States. The New England States and the extreme Southwest escaped.

Reports to the Weather Bureau show that 104 persons lost their lives between January 29th and February 13th from freezing and by avalanche (Colorado). The distribution was as follows: Colorado, 24; Texas, 15; Pennsylvania, 11; New York, 10; Illinois, 8; Missouri, 6; Ohio, 3; Maryland, 3; Iowa, 4; Wyoming, Delaware, Virginia, North Carolina, Alabama, Arkansas, Kentucky, and New Jersey, 2 each; Washington, South Carolina, and Georgia, 1 each. The usual reports of snow-bound trains, blockades in cities, and hardship from scant food and fuel supply were more widespread than in any other cold wave of which we have records in this country. The financial loss was incalculable. One of the most remarkable phenomena was the flow of ice down the Mississippi River on the 17th of February, past New Orleans and into the Gulf of Mexico—an event never before witnessed during the memory of man. Ice an inch thick formed at the mouth of the Mississippi, in East and Garden Island Bays, and the temperature fell to 10° on the 13th. Another phenomenon was the fall of snow at Jacksonville, Florida.

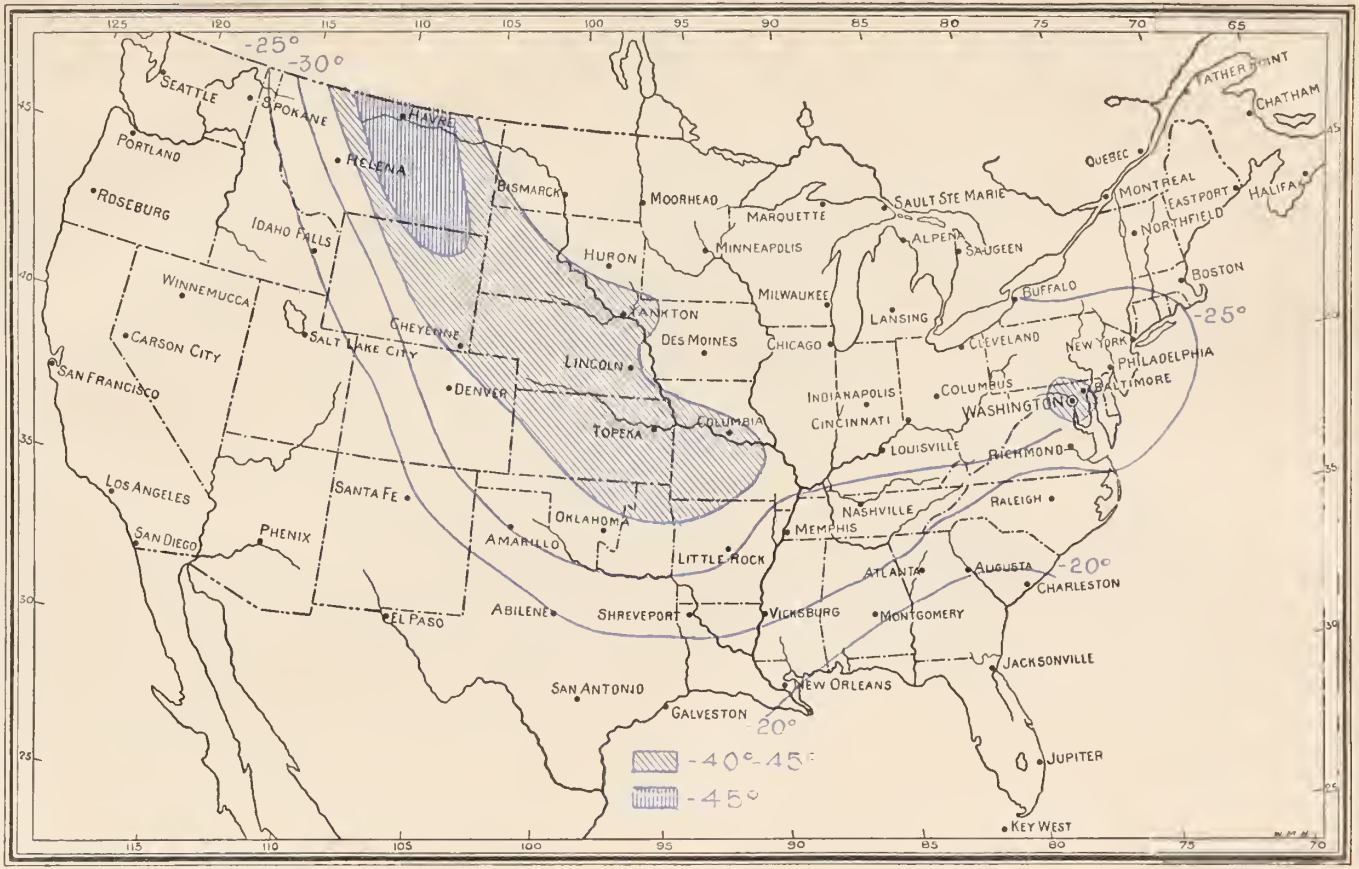
Here the temperature fell forty degrees in the twenty-four hours ending at 8 A.M. on February 9th, when a temperature of 10° was recorded, and the snow was from one to one and a half inches deep. Snow fell as far south as Titusville,

latitude 28° 32′, and Tampa; at Palm Beach the temperature fell to 28°; at Miami, latitude 25° 50′, it touched 29° on the 14th.

Fish and game birds were killed in great numbers by the extreme cold, and the loss of life among cattle on the Western plains was very great, owing to the depth of snow and the continued cold weather. Minimum temperatures of 25° below zero were observed at several places between Washington and Baltimore. The new record established for Washington was —15°, —7° at Baltimore, and —6° for Philadelphia.

The snowfall in Philadelphia began on February 11th at 9 P.M., and continued until the 14th at 1 A.M., during which time 18.60 inches fell. The wind was from the northeast, and blew at times at the rate of sixty miles an hour. The lowest barometer was 29.92 inches. The estimated monetary loss in Philadelphia was $2,500,000, and in the country at large $20,000,000. Food rose in price, and there was a shortage in meat, coal, milk, fresh vegetables, oysters, fish, and flour. The loss to railroads in receipts and in track clearing amounted in Philadelphia to about $1,000,000.

The vagaries of American climates were well illustrated in this storm. Some of the most striking phenomena were witnessed in Colorado, where several persons lost their lives by an avalanche. On February 12th the storm had lasted without cessation for nineteen days in the vicinity of Leadville. At Trinidad and many other points south of Denver the mercury fell 50° F. in two hours. At Colorado Springs the blizzard was accompanied by a fall of 35° F. in half an hour. At Topeka, Kansas, the Government thermometer at noon on the 11th registered 18° F. below zero. This is unprecedented for that time of day in Kansas Twenty-six degrees below zero was recorded in Omaha, Nebraska. In Huron, North Dakota, it was 36° F. below zero, and at Duluth, Minnesota, and La Crosse, Wisconsin, —32° F. On account of the absence of snow the ground in the vicinity of Chicago was frozen in many places to the depth of five and one-half feet.



Departures from Normal Temperature, February 11, 1899.

Departures from Normal Temperature, February 12, 1899.

Departures from Normal Temperature, February 13, 1899.

The President, Dr. Beverley Robinson: I feel that I am expressing the sense of all here when I say that we regret that we have not the time to fully discuss the papers we have just heard, in which are embodied the results of so much labor and research.

Before we adjourn, and before I declare this sixteenth annual meeting of the American Climatological Association at an end, I wish, in my last official capacity, to thank you all for your kindness and attention, and for the uniform courtesy with which you have treated me. Of course, you understand that if I have cut short the discussion, or prevented a member from occupying more time than belonged to him, I trust that he will appreciate that it was done only out of deference to your wishes. I also wish to say that we have reason to feel proud of the work done at this meeting. I think it would be difficult to attend any medical association and hear better papers, or those which give more earnest proof of work and investigation and all those qualities which will redound to the lasting reputation of this Association. I trust that what we have heard will inspire us with renewed energy, and that we will keep up the good work, so that at our next meeting in Washington the American Climatological Association will hold its own in every respect.

INDEX.

D indicates remarks in discussion of papers.

Air and light in Colorado, Gardiner, 19
Altitude, blood changes induced by, Solly, 144
Anders, Howard S., functional murmurs, D, 253
    influenza epidemic in Philadelphia, 1898–99, 286
Anderson, B. P., intermediate altitude for the consumptive invalid, 87
Aneurism of the aorta, T. Darlington, Jr., 218
Antecyclone, 304
Aortic aneurism, Darlington, 218
Arnold, Horace D., effects of violent exercise upon the heart, 267
Arrested cases of phthisis treated at Sharon, V. Y. Bowditch, 53
Atlantic City, dryness of soil and atmosphere, 102, 104, 107
Atmosphere in relation to skin, 128
Avalanche, loss of life by, in Colorado, 305

Babcock, R. H., delay in sending tuberculous patients away, D, 38
    high altitude and heart disease, 259
    linen underclothing, D, 124
    nitroglycerin for pneumonia, D, 195
    prognosis in valvular lesions of the heart, D, 185
Baths for skin disease, Ransom, 124
Bicycle accident to heart, autopsy, Newton, 226
Bicycling in its relation to heart disease, A. C. Getchell, 256
Blizzard, February, 1899, Hinsdale, 304
Blood changes due to running, 269
        induced by altitude, S. E. Solly, 144
Bonney, S. G., early diagnosis in pulmonary tuberculosis, 30
Bowditch, V. Y., empyema, D, 225
    subsequent histories of arrested cases of phthisis treated at the Sharon Sanitarium, 53
Bright's disease and climate, 99
Bulkley, L. D., relative value of linen and woolen clothing, D, 123
    renal disease treated by milk, D, 106
    skin diseases and climate, 108

Calomel in diphtheria, L. D. Judd, 197
Cannes, fumigation of apartments under municipal control, E. O. Otis, D, 98
Cardio-œsophageal gush and click, R. G. Curtin, 203
Clark, Sir Andrew, tribute to, 18
Climate as it affects the skin and its diseases, L. D. Bulkley, 108
Clothing, Judd, 118
Cold wave of February, 1899, Guy Hinsdale, 304
Coleman, T. D., calomel in diphtheria, D, 200
renal disease rare in Georgia, D, 103
Colorado air and light in treatment of consumption, 19
climate, B. P. Anderson, 88
Curtin, R. G., cardio-œsophageal gush and click, 203
horsebaek for pleuritie adhesions, D, 225
prognostieating heart disease, D, 184
rupture of the heart from a fall, D, 239
Cyclone, 304

Daland, Judson, cold bathing, D, 124
digitaline, D, 195
different foods required in different climates, D, 104
technique of blood examinations, D, 155
Darlington, Thomas, Jr., aneurism of the aorta, 218
calomel in diphtheria, D, 202
Davis, N. S., Jr., out-of-door life for consumptives, D, 28
prognosis in chronic valvular affections of the heart, 174
deaths from renal disease in various States, D, 106
Deaths by freezing, 305
Diagnosis in pulmonary tuberculosis, Bonney, 30
Diphtheria, calomel in, Judd, 197
Duckworth, Sir Dyce, 5

Electricity, static, for insomnia, I. H. Hance, 141
Elsner, H. L., empyema, D, 225
treatment of cardiac asthenia of pneumonia, 187
Embolism and prognosis in valvular heart disease, W. M. Gibson, 209
Empyema from a surgical standpoint, J. C. Munro, 220
Fisk, S. A., Bright's disease rare in the Rocky Mountain region, D, 105
diet in consumption as well as air, D, 29
Florida, freezing weather in, 306
Food, 104
Fumigation of apartments occupied by tuberculous patient at health resorts should be under municipal control, C. F. McGahan, 95
Functional cardiac murmurs, A. Jacobi, 240

Gardiner, C. Fox, light and air in the treatment of consumption in Colorado, 19
nature of apartments occupied by tuberculous patients, D, 97

Georgia, renal disease rare in, Coleman, D, 103
Getchell, A. C., bicycling and heart disease, 256
Gibson, W. M., the bearing of embolism on the course of valvular heart disease, 209
Grawitz theory of inspissation of blood, 148

Hance, I. H., hydrotherapy in the treatment of insomnia, 137
Heart disease and bicycling, A. C. Getchell, 256
effect of running on the, 272
effects of violent and prolonged muscular exercise upon the, H. Williams and H. D. Arnold, 267
murmurs, simulated, R. G. Curtin, 203
prognosis in chronic valvular affections of the, N. S. Davis Jr., 174
traumatic ruptures of, Newton, 226
Heat, effect on skin, 128

Hemoglobin and altitude, Solly, 144
Hemoptysis in early tuberculosis, 34
Heredity in tuberculosis, 33
High aims of the physician, Beverley Robinson, 1
Hinsdale, Guy, cold wave of February, 1899, 304
relation of hotel-keepers to the spread of tuberculosis, D, 97
Hotel-keepers and tuberculosis, 97
Hydrotherapy in the treatment of insomnia, I. H. Hance, 137

Infection, B. Robinson, 12
Influenza a cause of phthisis, 33
epidemic in Philadelphia, 1898–99, H. S. Anders, 286
Ingals, E. F., functional murmurs, D, 254
Insomnia, hydrotherapy for, I. H. Hance, 137
Intermediate altitude for the consumptive invalid, B. P. Anderson, 87

Jacobi, A., blood changes induced by altitude, D, 154
chloroform for children, not ether, D, 225
digitaline, D, 195
functional cardiac murmurs, 240
mineral waters for skin diseases, D, 117
Judd, L. da V., hygienics of the skin, 118
calomel in diphtheria, 197

Knight, F. I., early diagnosis of tuberculosis, D, 38
prognosis in valvular affections of the heart, D, 186
Röntgen ray examinations, D, 85
voice without larynx, D, 208

Lakewood, dry, 102
Langmaid, changes of the voice in early phthisis, D, 40

Langmaid, linen underclothing, D, 124
Leprosy in Hawaii and Minnesota, 111
Light and air in the treatment of consumption in Colorado, Gardiner, 19
effect on skin, 128
Linen mesh clothing, Judd, 121

McGahan, C. F., fumigation of apartments at health resorts, 95
Martins, of Rostock, on infectious diseases, 16
Meteorology and influenza, H. S. Anders, 286
Munro, John C., D, 254
empyema from a surgical standpoint, 220

New Mexico, Mesilla Valley for invalids, B. P. Anderson, 89
New Orleans, ice in, 306
Newton, R. C., dilatation of the heart, D, 285
diet for consumptives paramount, D, 93
kidney troubles rare in Texas, D, 104
laryngeal phthisis, D, 39
statistics as to renal disease in Texas, etc., misleading, 107
traumatic ruptures of the heart, 226
Nitroglycerin in pneumonia, 194

Œsophageal clicking, Curtin, 206
Oriental diseases, Bulkley, 111
Otis, E. O., blood changes induced by altitude, D, 154
early signs of pulmonary tuberculosis, D, 40
fumigation of apartments at health resorts (Cannes), D, 97
tuberculin test, 42
Oxygen in heart disease, 193

Phillips, W. F. R., blood changes induced by altitude, D, 156
light in the treatment of consumption, D, 28
relative dryness of Atlantic City and Philadelphia, D, 104
subnormal temperature in phthisis, D, 40
Pleurisy and tuberculosis, 34
Pneumonia, treatment of cardiac asthenia of, 187
Precipitation in Philadelphia, 298
Pulse after racing, 270

Quimby, Charles E., pneumatic differentiation in the treatment of heart disease, D, 171

Ransom, C. C., effect of bathing on the skin, D, 124
hydrotherapy for insomnia, D, 142
mineral water baths for skin diseases, D, 117
Relative humidity in Philadelphia in December, 297

Renal disease and climate, J. B. Walker, 99
Respirations after racing, 271
Robinson, W. D., climatology of nudity, 128
light and elevation, D, 28
Robinson, Beverley, the high aims of the physician, 1
Roe, John O., laryngeal appearance a sign of incipient phthisis, D, 40
Röntgen ray examinations in incipient pulmonary tuberculosis, F. H. Williams, 68
in the diagnosis of tuberculosis, 31
Rosenquist, blood cells, 146.

Sewall, Henry, altitude and heart disease, D, 170
renal disease and climate, D, 105
Sharon Sanitarium, result of treatment in, V. Y. Bowditch, 53
Skin disease and climate, L. D. Bulkley, 88
hygienics of, L. da V. Judd, 118
Snow blockade, 306
Solly, S. E., blood changes induced by altitude, 144
lower levels of New Mexico and Arizona for consumptives, D, 92
Stimulants in heart disease, 193
Stubbert, J. Edward, relative value of the fluoroscope and fluorescent screen, D, 85
Syphilis in Scotland, 112

Temperature in Philadelphia, 295, 306
Tornado, 304
Tuberculin test, E. O. Otis, 42

Virchow, 15

Walker, J. B , climate and renal disease, 99
linen mesh clothing, D, 124
nitroglycerin in pneumonia, D, 195
prognosis in heart lesions, D, 184
Weber, Leonard, use of various altitudes in treating phthisis, D, 92
Weight, loss of, in racing, 271
Williams, F. H., Röntgen ray examinations in incipient pulmonary tuberculosis, 68
Williams, Harold, anæsthetics for children, D, 224
effects of violent exercise upon the heart, 267
functional murmurs, D, 254
Wind velocity and direction in Philadelphia, 298
Winternitz, 148

Zuntz on blood-cells, 148